LABOUR MARKET OUTCOME OF1976 UNIVERSAL PRIMARY EDUCATION IN NIGERIA.

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ABSTRACT

In this study attempt to estimate the impact of schooling attainment on an important indicator of labour market performance: wealth. OLS and IV regressions produced economically and statistically significant estimates, with OLS estimate of about 18 percent and IV estimate of about 30 percent when pooled DHS is used and about 56 percent when HNLSS data are employed. We have no evidence that OLS estimates are an artefact of the way the dependent variable is constructed or influential observations are driving observed outcome. In our IV regression specification, econometric tests prove that instrument is strong. Indicative and formal tests of instrument validity such as addition of new relevant variables, falsification tests, plausibly exogenous test and over-identification test are proofs of instrument validity. A number of econometric strategies implemented indicate that influential observations and selective migration are not biasing our results.

Key Words: Schooling, OLS, IV, economic wellbeing

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1.0 Introduction

In 1976 the Federal Government of Nigeria (FGN) implemented nationwide Universal Primary Education (UPE) programme with the construction of over 21,000 new primary schools. The program was initiated to promote economic development and reduce regional inequality across North-South divide of Nigeria. Aside this**,** Lockheed and Verspoor (1991;1-10) assert that the programme is expected to produce a number of positive individual and social benefits, which among other things should lead to increase labour market performance of programme beneficiaries.

General evaluation of the programme by Taiwo (1980), Kosemani and Okorosaye-Orubite, (1995), Sanni and Ogonor, (2000), Nduka, (2000) Obanya, (2000 2002), Okobiah (2002) Jaiyeoba (2007) and Okorosaye-Orubite (2008) have not produced unambiguous results about programme's success. Other studies (Bray, 1981; Sunal et al. 1989) address concerns about the quality of education received by beneficiaries. The recent studies (Osili and Long, 2008; Osili 2008; Maret, 2012) employing econometric approaches have shown that programme help to reduce fertility, child mortality, increased female labour force participation and greater female empowerment. As far as we know, no study has explored the labour market performance of UPE beneficiaries. Thus, we extend this econometric approach to investigate the labour market consequences of the UPE programme by using the exposure to the programme and its implementation intensity to instrument for schooling. Therefore, this study uses pooled 2008 and 2013 Nigeria's Demographic and Health Survey (NDHS) to examine the causal impact of the programme on wealth through its influence on individual educational attainment. Thus, we are able to produce unbiased and consistent Local Average Treatment Effect (LATE) estimates from our IV strategy. A recent study by the author and other studies such as Osili and Long, (2008), Osili (2008) and Maret (2012) have shown that the programme has positive and significant impact on schooling attainment of beneficiaries.

To minimize self-selection bias due to non-random missing observations on individual income or deliberate misreporting of income to data collectors, we resort to the use of household assets as an indicator of labour market performance. We compute wealth variable from these household durables using standard market prices supplied by National Bureau of Statistics (NBS). An alternative approach uses Principal component analysis to compute another labour market performance indicator[2]. To avoid the confounding effects of Post-UPE Universal Basic Program that was initiated in 1999 and made compulsory in 2004, we limit our sample to those in 22-95 age brackets. In later specifications, we eliminated southerners who probably benefitted from previous regional UPE program that was widely implemented in the two regions of the south in the 1955-1966 period (Fafunwa, 1974).

First, and in order to provide baseline estimates against which we can compare with IV results, we present Ordinary Least Squares (OLS) results of the impact of schooling on wealth. Our results show that a year of schooling is significantly correlated with wealth. A year of education increases wealth by 20 percent. These estimates are robust to the inclusion of several variables, which might affect our indicator of wealth status. Because we are not sure that omitted variables that might bias upward OLS estimates, we use alternative survey data and IV empirical strategy, which allows us to check the extent to which omitted variables might be affecting OLS results. With the alternative data, we are to control for important variables such as vocational training, adult education, parental education, migration, school quality, natural ability, religion, political affiliation in our IV specifications.

[2] Using the DHS constructed as the main wealth indicator while desirable raises serious issue of interpretation.

To evaluate the wealth effect of the UPE program, we resort to IV strategy that exploits the accident of exposure to the program itself. This is aside the fact that the IV identification strategy ameliorates potential biases from OLS regressions due to endogeneity and measurement error problems. Since the official school entry age is six at the outset of the programme, we assume that those born between 1970 and 1974 (that is those within 2-6 age bracket in 1976) are UPE beneficiaries. Thus, UPE instrument is an indicator variable for cohorts exposed to the programme. Our IV result shows that a year of schooling increases wellbeing by 30 percent when we use the pooled DHS datasets and 57 percent when we use 2009 Harmonized National Living Standard Survey (HNLSS). These estimates are significantly higher than the OLS estimate. Results are robust to the inclusion of time invariant and time varying variables, which might confound our estimates of schooling variable. The IV estimate remains significant at 1 percent when a number of robustness tests were implemented. The F-statistic from the first stage regression shows that our instrument set is very strong. This study also shows that selective migration does not constitute a major source of bias for OLS and IV estimates. Influential observations do not appear to be responsible for observed outcome.

To address concerns about validity of our instruments, we developed two other placebo UPE laws and performed some falsification tests based on these placebo laws. There is no significant correlation between each of these false instruments and wealth. Nigeria is probably one of few countries in Sub-Sahara Africa (SSA) that introduced UPE programme in the mid-1970. Thus, if we use data from other African countries and assumed falsely that they implemented UPE in 1976 and operated it nationwide until 1981, the relationship between the placebo UPE instrument and wealth index should be zero and insignificantly different from zero. That exactly was what we found. The coefficients of the UPE instruments of other African countries turn out to be statistically insignificant.

Observed results cannot be attributed to pre-UPE schooling attainment trend. We fail to find evidence that rising household and national prosperity created the demand for the programme. Macroeconomic conditions at the time of labour market entry or entry before or after 1986 cannot responsibly account for observed wealth. We constructed an alternative instrument based on the assumption that school entry age is 5 years. We did this in order to implement direct test of overidentification. Our over-identification test fails to reject the exogeneity of main instrument. The paper also shows that the UPE programme did not have any negative impact on non-beneficiaries, eliminating concerns that general equilibrium effects of the programme might be substantially negative.

The rest of this paper is divided as follows. Section 2 gives an overview of the UPE programme in Nigeria. Section 3 provides background descriptive statistics of some of the selected variables. In section four we present OLS results and addresses issues around the use of wealth as dependent variable. In section five, we present IV results. In section six, we address concerns about instrument validity using variety of additional econometric strategies. In section seven we discuss the results of previous studies in comparison with the one obtained here. We summarized in this study in section eight.

2.0 Overview of the UPE Programme in Nigeria

The Federal Government of Nigeria initiated the Universal Primary Education (UPE) programme in 1974 and began its implementation in 1976 (Obasi, 1997; Ozigi and Ocho, 1981). The objective of Nigeria's UPE program was to provide tuition-free universal primary school education for six years. The official school entry age for the UPE program was put at

six years by the federal government. Urban areas had most schools constructed at the initial stage of the programme (Ozigi and Ocho, 1981).

The objective of Nigeria's UPE program was to provide tuition-free universal primary school education for first six years of basic education. One important reason for initiating this programme was to bridge the large gap in the schooling attainments of people in the Southern and Northern parts of Nigeria. For instance, why there were 4,225 primary schools in Northern Nigeria in 1974, the south had 10,313 primary schools (Annual Abstract of Statistics, 1981). It was, however, during the military regime of General Olusegun Obasanjo that the UPE programme implementation began in 1976. The third National Development that overlaps the period of UPE implementation made provision for the expansion of classrooms. The plan made provisions for 107,505 primary school classrooms in the northern states and 35, 045 classrooms in the southern states (Osili, 2008). As a results of the UPE programme, pupils enrolment rose from 6 million pupils in 1976 to 12 million by 1980 (Francis, 1998). While the program was terminated in 1981 by the deferral government, by 1982 pupils' enrolments had risen to 14.5 million nationwide. It is noteworthy that post-1981, growth in pupils enrolment has not been as dramatic as it was during the UPE programme (Francis, 1998).

In the immediate pre-independence years, the major regions in the South started the implementation of free Universal Primary Education. Lagos, then the Federal Capital Territory, was not left out. However, the programme started in the western region of Nigeria. The implementation started in January 1955. Lagos as the Federal Capital Territory outside the control of South-West started its UPE programme in January 1957. A month later, the Eastern region launched its own UPE programme. The Northern section did introduce conditional UPE programme in 1958. It ordered similar UPE policy could be implemented by localities when qualified teachers are available. Thus, it is actually difficult to say whether what was implemented could be the equivalent of a UPE programme. For whatever it is worth, UPE programme in the North was nothing in magnitude to what was implemented in the various parts of the southern regions. Since 1999, the federal government has re-introduced the program as Universal Basic Education (UBE), extending free basic education to the first nine years of schooling and making it compulsory. There is also now pre-primary component of the basic education program. To avoid the confounding effects of UPE program implemented before or after the 1976 UPE program, we eliminated observations that because of age could have benefitted from these programs.

3.0 Data Description & Preliminary Analysis

The study uses essentially 2008 and 2013 pooled Demographic and Health Surveys (DHS). National population Commission (NPC) collects the data. In addition, we use 2009 Harmonized National Living Standard Survey (HNLSS) collected by National Bureau of Statistic (NBS) to provide complementary evidence on the impact of the UPE program on wealth. Like most micro data collected by (NPC) and (NBS), both HNLSS and (DHS) are collected using the multi-stage stratified random sampling method. These household data use the sampling frame provided by the National population Commission (NPC). It is based on 36 states, a federal capital territory (F.C.T) and 774 Local Government Areas (LGAs), which we will sometimes refer to as districts.

The 2009 HNLSS data collected by NBS in collaboration with the World Bank. It uses a two-stage sampling method. The survey involves the selection of 10 Enumeration Areas (EAs) from each district located in 36 states of the federation and the F.C.T and the selection of households from each Enumeration Area. It adds up to 100 households per district and 77, 400 households across the Nation. *The 2006* Housing and Population Census provided the appropriate sampling frame for the Enumeration Areas selected in the 774 LGAs for the 2009/2010 HNLSS. In a two-stage sample design process, the first stage involves the selection of enumeration areas and the second involves the selection of households. DHS and HNLSS provide data on the status of an average Nigerian household's covering issues of demography, health, Fertility behavior, Education and Skills/Training, housing and housing condition, migration, social capital, agriculture, asset ownership, crime & security consumption and expenditure. We used 2006-2009 General Household Survey (GHS) and 2010 National Literacy Survey (NLS) and other data when we needed additional evidence to support our results. From these data sources, we select a number of variables and constructed others while incorporating data from other data sources. Some of the variables we constructed include UPE exposure and LGA UPE intensity

The UPE exposure instrument is constructed using age of respondents in the pooled DHS and HNLSS to determine who benefitted from the UPE program. Assuming 6 years as age of school entry, we calculate that only those born between 1970 and 1974 benefitted from the program. Younger or older cohorts did not. Dummy variable is constructed by assigning 1 to 1970-1974 cohorts and zero to others. To obtain a measure of the intensity of program implementation, we constructed for each LGA the enrolment rate before the start of the programme. This is because funds were allocated to different districts based on pre-programme LGA enrolment rate.

Panel A Table 1.0 provides preliminary information on some of the key variables used in our regression analysis. In column 1, the means and standard errors of these variables are presented for sampled respondents 22 years and above. To prevent the confounding effects of Universal Basis Education (UBE) programme, we excluded from our regression analysis observations below 22 years. The Federal Government of Nigeria (FGN) restarted UBE program in 1999 and extended it to cover the first nine years of basic education. In the next two columns, we show some descriptive statistics for those born 1970-1974 and those born 1958-1963. The former benefitted from the 1976 UPE program by virtue of time of birth and the latter group did not. The last column shows the differences between the groups while we use T-Test to check for the statistical significance of the differences.

While the mean value of wealth is N**144016, it is N8920.6 more for those born 1970-74 than those born 1958-63. Similarly, the difference in the schooling attainment of the two groups is nearly two years (row two). The differences in wealth and schooling attainment for both groups are statistically significant at 1 percent. The central argument of this paper is that the 1976 UPE program accounts for difference in schooling attainment between UPE cohorts and non-UPE cohorts, and is affecting wealth through schooling. However, there are potential confounding variables that come to play. From Table 1.0, it is clear also that there are statistically significant differences between the two groups with respect to marital status, sector of residence and access to electricity. Greater number of UPE cohorts live in urban areas and have better access to electricity, though smaller proportion are married relative to 1958-83 cohorts. UPE cohorts apparently do not have older co-ethnics who have invested more in schooling relative to the comparison group. There is also no pronounced differences between the two groups when we consider gender and access to water variables. We provide detailed regression analysis and the**

accompanying diagnostics to reinforce the central argument that UPE program, by exogenously raising the schooling attainment, increase wealth considerably. Our specification control for a broad range of variables.

Table 1A: Descriptive Statistics

Panel A				
Variables	Respondents 22 Years& Above (A)	Respondents Born 1970—1974 (B)	Respondents Born 1958—1963 (C)	Difference Between B & C 99 Percent CI
Wealth (In Naira)	144016 (210929.5)	154648 (217219.9)	145727.4 (219422.2)	N8920.6* (2500.385)
Schooling	5.993609 (5.766693)	6.687332 (5.659697)	4.819238 (5.696347)	1.868094* (0.0648545)
Gender Dummy	0.4853043 (0.5023088)	0.4996595 (0.5000154)	0.49101 (0.4999359)	0.0086495 (0.0056729)
Age	41.25956 (15.40607)	38.21826 (2.759077)	49.95054 (3.203038)	-11.73228* (0.0338214)
Sector Dummy	0.3644664 (0.4812819)	0.3688641 (0.482512)	0.3405521 (0.4739107)	0.028312* (0.0054281)
Marital Status	0.8562172 (0.3508706)	0.9518613 (0.2140659)	0.9881653 (0.1081455)	-0.036304* (0.0019101)
Ethnic Capital	1.854667 (0.907525)	1.826958 (0.8943965)	1.844268 0.9206987	-0.01731 (0.0103438)
Time to Get to Water Source (in minutes)	261.5605 (417.6454)	262.1999 (418.2074)	261.1251 (416.7968)	1.0748 (4.73746)
Electricity Access Dummy	0.5070478 (0.4999521)	0.5102041 (0.4999114)	0.4812085 (0.4996635)	0.0289956* (0.0056771)

Population variances of samples in columns 1 & 2 are assumed to be unknown. However, we assumed equal variances for the two samples when their standard deviations are not significantly different from each other. *Significant at 1 percent.

For a study of this kind the dependent variable should normally be income, or to be precise earnings per hour. In a developing country setting, this is not an information most sampled respondents would be willing to divulge, aside the fact that accuracy of the information may be in doubt. For GHS data that collects data on individual earnings in Nigeria, there are considerable number of missing data in a non-random manner. This exacerbates the inherent self-selection biases in studies of this kind. In Panel B Table 1.0, we show for selected variables the descriptive statistics for those who are between 22 and 64 years. The choice of 22-64 cohorts is guided by the need to exclude UBE cohorts and older respondents outside the labour force. Column 1 reports the means and standard errors of these cohorts, and in columns 2 and 3 split these cohorts in those who report the income and those did not.

Column 4 shows whether for each of the variables listed in the table, there are statistically significant differences between those who report and those who did not report their income. As

seen from Panel B Table 1.0, there are statistically significant differences in the characteristics of the two groups.

Table 1B: Descriptive Statistics

Panel B: 2006-09 GHS				
Variables	Respondents Between 22 & 64 Years (A)	Respondents With Income Data (B & C)	Respondents Without Income Data (C)	Difference Between B & C 99 Percent CI
Income Per Hour (In 1,000 of Naira)	0.4738115 (5.95927)	0.4779064 (5.984803)	NA	NA
Schooling (years)	9.657765 (4.108789)	9.362127 (4.123832)	9.996088 (4.065267)	-0.633961* (0.029029)
Age	38.01984 (11.16669)	40.87507 (10.86344)	35.17301 (10.72962)	5.70206* (0.0576194)
Gender Dummy	0.4772387 (0.4994834)	0.6255598 (0.4839815)	0.3293269 (0.4699722)	0.2962329* (0.0025461)
Sector Dummy	0.251141 (0.433671)	0.2631849 (0.4403651)	0.2391326 (0.4265569)	0.0240523* (0.0023136)
Self-employment	0.012311 (0.1102704)	0.0207935 (0.1426935)	0.0038535 (0.0619576)	0.01694* (0.0005873)
Paid-Employment	0.0766076 (0.2659687)	0.1263584 (0.3322552)	0.0270032 (0.1620938)	0.0993552* (0.0013957)
Unemployment (During the Year).	0.0155493 (0.1237244)	0.0119919 (0.10885)	0.0251448 (0.1565681)	-0.0131529* (0.0010784)
Radio (Number)	1.014629 (0.6948219)	0.9536371 (0.6219025)	1.170468 (0.833275)	-.2168309* (0.0073194)
TV (Number)	0.9389465 (0.8020063)	0.8143608 (0.7008354)	1.189331 (0.9246372)	-0.3749702* (0.0095651)
Mobile (Number)	0.9456909 (0.6935945)	0.8450388 (0.6089889)	1.135711 (0.7961952)	-0.2906722 (0.0069906)
Fixed Phone (Number)	0.6982298 (1.060156)	0.2254023 (0.8033865)	1.198685 (1.069288)	-0.9732827* (0.0137478)
PC (Number)	0.7148858 (1.065525)	0.280271 (0.8886165)	1.188778 (1.040113)	-.908507* (0.0139063)
UPE Exposure	0.139003 (0.3459509)	0.1457115 (0.3528193)	0.1323143 (0.3388345)	0.0133972* (0.001846)

Population variances of samples in columns 1 & 2 are assumed to be unknown. However, we assumed equal variances for the two samples when their standard deviations are not significantly different from each other. NA is not applicable. *Significant at 1 percent.

The more educated and younger cohorts are less willing to report income. Greater proportion of those living in rural areas and those in paid employment are less inclined to report their income relative to those in urban areas and those in self-employment. Thus, those with regular stream of income, which can be estimated accurately are less likely to report their earnings compared to those with irregular income and whose incomes are difficult to estimate accurately. It is also noteworthy that greater percentage (2.5 percent) of those not reporting income report some unemployment spell in the course of the last one year than those who report their incomes (1.19 percent). Since a substantial proportion of those in self-employment are engaged in farming and

other vocations that have considerable seasonal component, reported incomes from this category of workers might come with some noise. They also turn out to be wealthier in terms of ownership of assets such as radio, television, personal computers, mobile and fixed phones. However, slightly greater number of UPE beneficiaries are willing to report their incomes than beneficiaries those not willing. The pronounced differences between these two groups could partly explain why there are significant differences in regression estimates that use wealth and earnings as dependent variables. OLS results for regressions using earnings are significantly lesser than those using wealth. Considerable number of missing non-random observations makes our UPE instrument to be extremely weak, and estimates of schooling variable completely unreliable.

4.0 Econometric Model and Empirical Results

4.01. OLS Estimates

We begin by estimating the relationship between the years of schooling the and indicator of well-being. Our baseline estimating equation (1) is:

$$W_{itk} = \beta_1 + \beta_2 S_{itk} + \beta_3 X + \delta + \theta + \epsilon \qquad (1)$$

where W_{itk} the natural logarithm of the monetary value of the wealth of individual **i** at time t resident in LGA **k**. Market price information supplied by NBS is used to compute individual wealth, summing up the monetary value of all assets reported in both 2008 and 2013 NDHS. Similarly, Sisk is the years of schooling of individual **i** resident in state **s** and LGA **k**. In addition, we have δ as a set of state, LGA and geopolitical region fixed effect variables to pick unobserved variables driving outcomes aside schooling attainment and other control variables (X). .

From the column 1 of Panel A Table 2.0, schooling attainment has strong and positive impact on economic wellbeing. On the average, an extra year of education increases wealth status by 24.23 percent. This translates to about N6756.23 rise in wealth for an additional year of schooling. When variables such as age and age-squared are included in regression reported in column 2, sector dummy to column 3, gender dummy to column 4, household size to column 5 and marital status dummy to column 6, coefficient of schooling attainment reported in column 1 declines marginally to 22.05 percent. When ethnic capital variable is added to the regression reported in column 7 and time-dummy is added to that in column 8, estimate of schooling variable reduces marginally to 20.39 percent.

In column 9, we regress logarithm of individual income per hour on schooling attainment, gender dummy, age, age-squared, sector dummy and time using pooled 2006-2009 GHS datasets. The estimate for schooling variable is 3.24 percent, considerably smaller than estimates reported using wealth variable. However, the estimate is statistically significant though has minimal economic significance. This is probably because of the income data, which is missing for a non-random fraction of the sample. Running separate regressions for GHS 2006, 2007, 2008 and 2009 did not yield any economically meaningful estimates for schooling variable, except for GHS 2009, which is 12.00 percent, though statistically insignificant at 10 percent (columns 1-4 appendix 2). Similarly, separate regressions for 2008 and 2013 DHS with wealth as dependent variable yield economically and statistically significant estimates (columns 5-6 appendix 2), which in quantitative terms are similar to

TABLE 2.0: OLS Regression Results Dependent Variable (Log of Monetary Value of Wealth & Log of Earnings Per Hour for Column 9).

Variables/column	1	2	3	4	5	6	7	8	9
Survey	**2008 & 2013 DHS**	**2008 & 2013 DHS**	**2008 & 2013 DHS**	**2008 & 2013 DHS**	**2008 & 2013 DHS**	**2008 & 2013 DHS**	**2008 & 2013 DHS**	**2008 & 2013 DHS**	**2006-09 GHS**
Dependent Variable	**Wealth (Log)**	**Wealth (Log)**	**Wealth (Log)**	**Wealth (Log)**	**Wealth (Log)**	**Wealth (Log)**	**Wealth (Log)**	**Wealth (Log)**	**Income Per Hour (Log)**
Schooling (years)	**0.2423*** (0.0015)**	**0.2377*** (0.0016)**	**0.1964*** (0.0017)**	**0.1987*** (0.0017)**	**0.2159*** (0.0017)**	**0.2205*** (0.0017)**	**0.2080*** (0.0019)**	**0.2039*** (0.0018)**	**0.0324*** (0.0062)**
Age & Age2	**No**	**Yes**	**Yes**	**Yes**	**Yes**	**Yes**	**Yes**	**Yes**	**Yes**
Sector Dummy	**No**	**No**	**Yes**	**Yes**	**Yes**	**Yes**	**Yes**	**Yes**	**Yes**
Gender Dummy	**No**	**No**	**No**	**Yes**	**Yes**	**Yes**	**Yes**	**Yes**	**Yes**
Household Size & Household 2	**No**	**No**	**No**	**No**	**Yes**	**Yes**	**Yes**	**Yes**	**no**
Marital status	**No**	**No**	**No**	**No**	**No**	**Yes**	**Yes**	**Yes**	**Yes**
Ethnic Capital	**No**	**No**	**No**	**No**	**No**		**Yes**	**Yes**	**No**
Time-Dummy	**No**	**No**	**No**	**No**	**No**	**No**	**No**	**Yes**	**Yes**
Observations	**137503**	**137372**	**137372**	**137372**	**137372**	**135652**	**134748**	134748	45520
F-Statistics	**25257.38**	**8568.06**	**8171.98**	**6571.48**	**5213.66**	**4526.86**	**4021.87**	**3765.23**	**31.29**
R-Squared	**0.1376**	**0.1395**	**0.1666**	**0.1668**	**0.1974**	**0.1981**	**0.2001**	**0.2089**	**0.0046**

Robust standard errors are expressed in brackets. ***, **, and * indicate significance at the 1%, 5%, and 10% levels. **Age2 is age-squared. Household 2 is household size squared.**

estimates reported in Table 2.0. In case omitted variables are driving estimates, we control for geopolitical region, state and LGA fixed effects separately in regressions reported in columns 1, 2, and 3 of appendix 3. All fixed effects variables are added in column 4. If geographic factors are important in determining wealth, adding variables such as LGA latitude, longitude and altitude could lead to drastic reduction in the estimate of schooling variable. However, as seen in columns 5-7 Appendix 3, adding these variables has barely any effect on the estimates of schooling variable.

We explore whether the impact of schooling attainment may differ along gender and sector dimensions. Results are shown in Appendix 4. In columns 1-2, we report regressions that account for potential heterogeneity along these lines. Columns 1 and 2 show estimates for male and female sample respectively. Both estimates are positive and significant at 1 percent, with higher estimate reported for female sample. A year of schooling raises wealth by 17.8 percent for a male and 20.87 percent for female. Rural sample in column 3 (19.72 percent) is higher than urban estimate reported in column 4 (17.63 percent). Estimates for rural and urban samples are significant at 1 percent.

However, different parts of the country have varying degree of urbanization that could be influencing reported estimates. Thus, locations such as Lagos and Abuja have greater urban representations. Similarly, all states from the South-West of Nigeria have greater urbanization level than other locations. The unequal distribution of amenities and opportunities across space could bias OLS estimates. To correct for this potential source of bias, we exclude one after the other observations from FCT, Lagos and Abuja and South-West in regressions reported in columns 5-7 (Appendix 4).The estimates of schooling variable remain relatively unchanged. Estimates are still significant at 1 percent. Dropping each of the other state one at time has not affected the statistical significance of the schooling coefficients. Similarly, implementing OLS regressions for each of the 36 states and Federal Capital Territory (F.C.T) produced coefficients of schooling variables that are statistically significant at 1 percent. Similarly, limiting regression to 22-64 cohorts to check whether the inclusion of 65 years plus cohorts may be driving outcome has barely no effect on schooling coefficients[3].

4.0.2 Alternative Indicators of Wealth Index As A Dependent Variable

The use of wealth as dependent comes with a number of concerns. It is also possible that household assets could have been received as gifts, and thus unrelated to the actual economic status of individuals. Household head may exclusively owned assets assigned to all individuals within the household, and in some cases jointly owned with the spouse. Analysis of the 2012 Panel General Household Survey data collected by National Bureau of Statistics (NBS) reveals that the household head owns 95 percent of durable assets in the home, another 4 percent by the spouse and the remainder jointly owned by all members. To deal with these concerns, we estimate a number of alternative specifications with different dependent variables. We adopt four different approaches.

First, we selected assets that are more likely to be owned by individuals in the home. This includes motorcycle, car and mobile phones. In columns 1-3 Table 3.0, we show the results of probit regressions. Schooling is still significantly correlated with the likelihood of possessing a motorcycle, a car and a mobile phone. Two, we use the wealth index produced

[3] Results not shown but available upon request.

Table 3.0 Alternative Indicators of Wealth As Dependent Variable

Model	1	2	3	4	5	6	7
Assets	**Motorcycle**	**Car**	**Mobile Phone**	**Wealth Index**	**Wealth ranking**	**Wealth (Household Head Only)**	**Wealth (One-Person Household)**
Schooling (years)	**0.0189*** **	**0.0915*** **	**0.0425*** **	**7975.3*** **	**0.0989*** **	**0.2611*** **	**0.2527*** **
	(0.0008)	**(0.0012)**	**(0.0019)**	**(43.57)**	**(0.0006)**	**(0.007)**	**(0.0079)**
Other control variables	**Yes**	**Yes**	**Yes**	**Yes**	**Yes**	**Yes**	**Yes**
Regression Technique	**Probit**	**Probit**	**Probit**	**OLS**	**OLS**	**OLS**	**OLS**
Observations	**136626**	**136627**	**136475**	**137222**	**137222**	**13998**	**9613**
Wald chi2(16)/ F-Stat	**9827.44**	**11734.51**	**2378.94**	**16979.82**	**18627.88**	**421.79**	**358.26**
Pseudo R2/ R-squared	**0.0598**	**0.1740**	**0.0969**	**0.6123**	**0.5943**	**0.3355**	**0.3775**

Other control variables include age, age-squared, sector dummy, gender dummy, household size, household size squared, marital status dummy, ethnic capital and time dummy, fixed effects for geo-political region, state, LGA, LGA Latitude, LGA Longitude and LGA altitude. NA is not available. Robust standard errors are expressed in brackets. ***, **, and * indicate significance at the 1%, 5%, and 10% levels.

DHS body. This index is derived from a principal components analysis of all households' assets and housing characteristics of sampled respondents. From column 4, it is clear that schooling is significantly correlated with DHS wealth index at 1 percent level. In columns 6 and 7, we return to the use of previous wealth variable computed from NBS market price information chart. The column 6 regression is restricted to household head only sample and column 7 to one-person household sample. In both cases, schooling variable remain statistically significant at 1 percent, rising modestly to 26.11 and 25.27 percent for columns 6 and 7.

5.0 Instrumental Variable Strategy

5.0.1 Preliminary Background Analysis

Reported OLS regressions may not have convincingly handled the omitted variables problems, just as endogeneity of schooling variable and measurement error are still important sources of biases. More importantly, OLS regressions do not constitute an evaluation of the UPE program in terms of wealth effect. Our final strategy is to use IV method to resolve these potential sources of bias. The IV strategy allows us to estimate the wealth returns to schooling for UPE beneficiaries.

Our IV identification technique depends on the exposure to UPE program, which depends largely on year of birth if we assume the school entry age is 6 years. For those born between 1970 and 1974, they fall within the category of beneficiaries because they were just within 2-6 age range when the program commenced in 1976. A comparison group born 1958-1963 were between 13-18 age ranges in 1976, and could not have benefitted from the program. Indicator variables should have differential impact on schooling attainment and wealth for UPE and comparison groups. If we constructed 11 indicators for year of birth (6 for 1958-1963 cohorts and 5 for 1970 and 1974). We show the results in Table 4.0 after regressing schooling attainment and wealth on these 11 indicator variables and other covariates.

Generally, year of birth indicator variables for 1958-1963 cohorts have negative and statistically significant effects on schooling attainment and wealth of sampled respondents, while indicator variables for 1970-1974 have positive and statistically significant effect on schooling and wealth (columns 1-4). In columns 2 and 4, we see how much less schooling and wealth each of the pre-UPE cohorts have relative to UPE cohorts. We also see how much more schooling and wealth UPE cohorts have relative to pre-UPE cohorts. In fact, UPE cohorts have nearly 0.5 year more schooling and 17.17 percent more wealth relative to 1958-1963 cohorts. The UPE program effect on treated cohorts and cohorts just before the program started is a kind of event-study regression analysis that shows the impact of program before and during the event. It shows that trend of schooling attainment across cohorts might not be driving our results. Columns 5 and 6 reveal what happens when we account for the impact of schooling attainment in a reduced-form regression with wealth as a dependent variable. Most of the 11 year of birth indicator variables become statistically insignificant when schooling attainment variable is introduced into the specifications (column 5). In the same vein, the UPE indicator becomes insignificant as schooling variable is added into the specification (column 6). This provides some preliminary evidence in support of exclusion restriction condition, which our UPE exposure instrument must

satisfied for IV regression to generate consistent estimates for schooling variable. Results of reduced-form regression reported in Table 4.0 indicates that exposure to UPE program might be a good instrument. Later, we provide additional evidences to show that our instrument is at least plausibly exogenous. It is also unlikely weak instrument related bias is of serious concerns in this study.

TABLE 4.0: Background Reduced Regressions (The Event Style-Study Regressions)

Model	1	2	3	4	5	6
Dependent Variable	Schooling Attainment	Schooling Attainment	Log of Wealth	Log of Wealth	Log of Wealth	Log of Wealth
Schooling Attainment	No	No	No	No	Yes	Yes
Born 1958 Indicator Variable.	-1.532*** (0.2377)	-2.030*** (0.1917)	-0.393*** (0.0982)	-0.6238*** (0.1381)	-0.1689* (0.0955)	-0.2482* (0.1336)
Born 1959 Indicator Variable.	-0.6855*** (0.2406)	-1.135*** (0.2054)	-0.1331 (0.1156)	-0.3342** (0.1437)	-0.0275 (0.1108)	-0.0947 (0.1378)
Born 1960 Indicator Variable.	-0.6284*** (0.2053)	-1.024*** (0.1733)	-0.0081 (0.0936)	-0.1817 (0.1199)	0.0792 (0.0899)	.0266496 (0.1152)
Born 1961 Indicator Variable.	-0.5248*** (0.1874)	-0.8571*** (0.1630)	0.0095 (0.094)	-0.1357 (0.1121)	0.0785 (0.0902)	0.0328 (0.1080)
Born 1962 Indicator Variable.	-0.2091 (0.1915)	-0.4958*** (0.1742)	0.0359 (0.1079)	-0.0822 (0.1200)	0.0569 (0.1031)	0.0292 (0.1146)
Born 1963 Indicator Variable.	-0.8890*** (0.1296)	-1.1116*** (0.1128)	-0.145 ** (0.0703)	-0.2376** (0.0816)	0.00567 (0.0683)	-0.0105 (0.0789)
Born 1970 Indicator Variable.	0.6165*** (0.1296)	No	0.1363** (0.0687)	No	0.0355 (0.0663)	No
Born 1971 Indicator Variable.	0.4703*** (0.1481)	No	0.2024*** (0.0644)	No	0.1411** (0.0619)	No
Born 1972 Indicator Variable.	0.4281** (0.1763)	No	0.1185 (0.0773)	No	0.0833 (0.0749)	No
Born 1973 Indicator Variable.	-0.3519** (0.1702)	No	0.1840*** (0.0460)	No	-0.0730 (0.0448)	No
Born 1974 Indicator Variable.	0.9119*** 0.2154155	No	0.2982*** (0.0685)	No	0.1859** (0.0666)	No
UPE Indicator	No	0.4987*** (0.1161)	No	0.1747** (0.0799)	No	0.0795 (0.0770)
Control Variables	Yes	Yes	Yes	Yes	Yes	Yes
Observations	71208	71208	70570	70570	69857	69857
F-Statistics	1947.04	2221.63	521.05	467.65	705.64	658.75
R-Squared	0.3664	0.3649	0.1541	0.1545	0.2162	0.2163

Other control variables include age, age-squared, sector dummy, gender dummy, marital status dummy, ethnic capital and time dummy, fixed effects for geo-political region, state, LGA, LGA Latitude, LGA Longitude and LGA altitude. Robust standard errors are expressed in brackets. ***, **, and * indicate significance at the 1%, 5%, and 10% levels.

5.0.2 Instrumental Variable (IV) Results

In the second-stage regression results shown in Panel A Table 5.0, and starting with bivariate regression indicated in column 1, estimate show a positive and highly significant effect of the schooling attainment on wealth. A year of schooling increases individual wealth by **43.55** percent, which is significant at 1 percent. This is nearly twice as high as OLS estimates reported in column 1 of Panel A Table 2.0. In column 2 Panel A Table 5.0, we introduce variables such as age, age-squared and dummies for sector, gender and marital status as additional controls. Estimate of schooling declined dramatically to 19.35 percent, but is still significant at 1 percent.

Previous investment of various ethnic groups within the polity partly determine the current schooling attainment adult population. Ethnicities with poor prior records of human capital investment could be more or less willing to participate in the UPE program than other ethnic groups. Thus, the UPE instrument could be correlated with ethnic capital, producing bias estimates. To remove this potential source of bias, we control for ethnic capital, estimated as the average schooling attainment of 65-95 cohorts for each of the ethnic groups identified in the DHS data. Column 3 shows that the schooling estimate increased slightly, from 19.35 percent to 18.34. To column 4 we add time dummy. This increased the schooling estimate to 28.98 percent from 18.34 percent while it is still statistically significant at 1 percent.

Before the UPE programme, government committed considerable resources in physical projects, such as network of roads, electricity and dams for irrigation and water supply. A considerable fraction of these projects took place the Gowon administration and subsequent administrations that implemented the UPE program. These investments could have independent effects on wealth or might even after wealth through schooling. We introduce infrastructure variable measuring the extent to which respondent is close to water source measured in minutes and availability of electricity within the household. Schooling estimate dropped modestly to **23.07** percent (column 5) after adding infrastructure variables.

In column 6, we control for the fact that time invariant unobserved state level factors, like state policies that permanently created differences in wealth fortune of those resident in them. Embedded traditional attitudes and practices at the LGA level may have permanent effects on labour market performance. State fixed effects variable is introduced to check the magnitude of these factors on observed outcome. We acknowledge the possibility that some time invariant factors could work the geopolitical zone, and needed to be accounted for. After accounting for time invariant factors at different levels, the schooling coefficient is still significant at 1 percent, increasing modestly from 23.07 to 27.56 percent.

Geographic locations, which support higher level of economic activities, could increase incentives to participate in the UPE programme because returns to schooling will be much higher than those living in geographic location with comparatively lower volume of economic activities. There might also be difference in participation in the UPE programme because of the topographic features of each location. For instance, some LGAs could have highly irregular terrains, making schools more difficulty to reach for relevant cohorts. Apart from this, geographic characteristics could have independent effect on wealth or economic performance (Nunn and Puga, 2012), violating the exclusion restriction condition. To mitigate the potential effect of geographic factors, we introduced LGA altitude to measure the topography of the LGA landscape. As shown in column 7, schooling variable remains significant at 1 percent, increasing marginally to 30.22 percent. For comparison, we produced

Table 5.0 IV Regressions Results

Panel A: Second Stage Results (Dependent Variable: Log of Wealth)								
Survey Sample	**NDHS**	**NDHS**	**NDHS**	**NDHS**	**NDHS**	**NDHS**	**NDHS**	**GHS**
Model	**1**	**2**	**3**	**4**	**5**	**6**	**7**	**8**
Dependent Variable	**Log of Wealth**	**Log of Wealth**	**Log of Wealth**	**Log of Wealth**	**Log of Wealth**	**Log of Wealth**	**Log of Wealth**	**Income per Hour**
Schooling (years)	**0.4355*** (0.0483)**	**0.1935*** (0.0454)**	**0.1834*** (0.0667)**	**0.2898*** (0.0619)**	**0.2307*** (0.0668)**	**0.2756*** (0.0800)**	**0.3022*** (0.0834)**	**1.1343 (3.2228)**
Panel B: First Stage Results (Dependent Variable: Schooling Attainment (Measured in Years)								
UPE_E	**0.7317*** (0.0480)**	**0.6430*** (0.0455)**	**0.4395*** (0.0426)**	**0.4770*** (0.0426)**	**0.4286*** (0.0407)**	**0.3608*** (0.0399)**	**0.3485*** (0.0394)**	**0.0337 (0.0507)**
Observations	**127931**	**126178**	**125324**	**125324**	**125202**	**125198**	**124656**	**45520**
F-Statistics	**229.35**	**12151.63**	**16509.32**	**14565.76**	**14430.39**	**12220.19**	**10568.85**	**0.4400**
Cragg-Donald Wald F statistic	**219.81**	**219.20**	**116.79**	**137.75**	**120.17**	**88.68**	**84.58**	**0.41**
Paap-Kleibergen F-Statistic	**229.35**	**199.79**	**106.60**	**125.62**	**111.05**	**81.81**	**78.13**	**0.4400**

MODEL 1 is a bivariate IV regression with schooling attainment the only explanatory variable. Other control variables in model 2 included age, age-squared, sector dummy, gender dummy and marital status dummy. Model 3 is model 2 plus ethnic capital. Model 4 is model 3 with time dummy added to it. Model 5 is model 4 plus infrastructure capital (electricity availability dummy and time to get to water source). Model 6 is model 5 in addition to LGA fixed effects, geopolitical and state fixed effects. Model 7 has all variables included in model 6 in addition to geographic variables (latitude, longitude and altitude). Control variables added to column 8 include age age-squared, gender dummy, sector dummy and marital status dummy. Robust standard errors are expressed in brackets. ∗∗∗, ∗∗, and ∗ indicate significance at the 1%, 5%, and 10% levels.

IV results with pooled 2006-2009 GHS datasets and using logarithm of income per hour as dependent variable. We obtain a highly imprecise 113 percent, economically large but statistically insignificant (column 8). Because of non-random missing income data, UPE instrument turn out to be very weak. This raises concern about the consistency of our results using GHS datasets.

There are genuine concerns that IV results reported might be due to how our UPE exposure instrument is constructed. It is likely alternative UPE instrument might produce radically different results. Five other alternative indicators of UPE exposure are constructed to check how robust our regression estimates are to alternative specification of UPE exposure variable.

The first alternative indicator is based on years of exposure to the programme. Those who are 6 years old in 1976 had five full years of UPE programme. Years of exposure to UPE declined by a year as one moves progressively towards the lower limits of 2-6 age bracket. The second instrument allows the school entry age to drop to 5 years to account for potential underage enrolment and third UPE instrument permit to the school entry age to be 7 years to accommodate overage enrolment. The fourth instrument simultaneously allows both underage and overage enrolment. The fifth instrument is based on the fact those who are 7-11 cohorts in 1976 also benefited from the UPE programme though they were already in school at the outset of the programme, and a considerable number could have completed basic education without the support of the programme. A sizeable percentage may not be able to complete basic education without the programme support. Empirical results are shown in columns 1-5 appendix 5. In all cases, schooling estimates are slightly or modestly higher than IV estimate reported in column 7 Table 5.0 and are significant at 1 percent.

IV estimates reported are unbiased and consistent if the instrument is strong. Weak instrument could bias estimate in the same direction as OLS (Bound, Jaeger and Baker, 1996). A number of diagnostic tests does not suggest we are likely to have bias arising from instrument weakness. First, in all first IV regressions reported, the UPE instrument is positively and significantly correlated with schooling attainment. Compared with the rule-of-the-thumb value of 10 given by Stock and Yogo (2002), all first-stage F-Statistic reported in in this study show that the instrument set is strong. We come to the same conclusion if we use Cragg-Donald Wald F statistic and Paap-Kleibergen F-Statistic. Additional tests of instrument strength follow the suggestion offered by Angrist and Pischke (2009). Using maximum likelihood IV estimation technique (LIML), which correct bias due to weak instrument yields basically the same estimate (column 6 appendix 5). We also use two Jack Knife estimators (JIVE) which can outperform LIML estimation technique when the instrument is weak (Poi, 2006). The two JIVE estimators are almost the same as the estimate reported for the original IV technique (Columns 7 and 8 appendix 5).

5.0.2 Checking for Influential Samples & Selective Migration

We check for the impact of influential samples as we did for our OLS regressions. Thus, we progressively drop observations from F.C.T in column 1, Lagos and F.C.T in column 2 and South-West in column 3 (appendix 6) using HNLSS data. We repeated the same process with DHS datasets (columns 4-6). While schooling coefficients remained significant at 1 percent, estimates drop sharply as shown in columns 1-3 when HNLSS is used but remain barely unchanged when DHS datasets are used. Results indicate that observations from these rapidly

developing regions are not necessarily driving results, with the schooling and wealth relationship remaining positive and significant at 1 percent.

Migration is just one important reason why doubt exists as to whether exclusion restriction could be fulfilled. Duflo (2004) identified two sources of sample selection bias. One, there is the possibility of selective migration along unobserved characteristics. This occurs if low productivity old people moved towards LGAs where the programme was implemented with greater intensity or high productivity old people quit living in the low programme intensity LGAs. Two, because the programme affected the segment of the sample population for whom specific outcomes are observed, the probability of selection in the sample is affected by the instruments. There is a similar possibility of bias if programme beneficiaries move to better neighbourhoods after acquiring basic education. The bias is more serious when beneficiaries move farther afield, and the sending and receiving regions are fundamentally different from each other. This happens when the movement is massively from rural locations to thriving urban centres. According to Schultz (1988) and Duflo (2004), selective migration might bias coefficients of schooling attainment variable towards zero.

If selective migration is prevalent, then it is likely that IV estimates will be biased. Selective mass migration may undo the effect of the UPE programme (Duflo, 2004). Thus, estimates of private or external returns to schooling investment will be incorrect when migration occurs (Schultz, 1988). When individuals educated in the rural area move to urban locations due to rural–urban wage differentials, wage returns are partly associated with education and migration, while returns to schooling in the rural sector is under–estimated. A study in Colombia, reported in Schultz (1988) on private returns to schooling discovers substantial difference in return to schooling between rural and urban sectors when migration is accounted for.

Though, there is evidence that across the state migration is not of a considerable magnitude to affect our schooling estimates (Osili and Long, 2008; Osili 2008; Oyelere, 2010), there are still concerns that within state migration might confound schooling estimates from IV regressions. Nigerian urbanization literature (Mberu, 2005) has reported a massive increase in the number of urban areas in the last three decades. If rural-urban migration within states is considerable schooling coefficient estimates will be biased in a model that fails to account for selective migration.

Furthermore, we used the 2009 HNLSS data to provide additional evidence for instrument validity. From this data, we simply added migrant dummy to the set of explanatory model included in column 7 (appendix 6). If selective migration is important for observed, including this variable should pick up the effect, leaving the coefficient of schooling attainment insignificant. Including migrant dummy leaves schooling coefficient essentially unaffected. Running regression for non-migrant sample produced slightly lower schooling coefficient (Column 8). However, the coefficient is still statistically significant.

5.0.3 IV Regression Results Robustness to Alternative Survey and Additional Variables

The positive correlation between schooling attainment and wealth status documented in the previous regressions is consistent with our expectation that UPE programme augments individual schooling attainment, and through schooling, raises individual wealth. Despite the fact that UPE instrument could mitigate omitted variable bias, there are concerns that the exclusion of some relevant variables may induce spurious relationship between schooling attainment and wealth. What is more, we computed wealth variable from 12 listed household assets, raising the possibility that higher income families may have greater number of household assets not covered in the two DHS surveys. This may bias our schooling attainment estimate downward. To preclude biases from these sources, we turn to the 2009 HNLSS data described previously. HNLSS data have 20 household assets listed as against 12 in DHS data.

As a first measure, we use produce IV regression using the complete set of covariates used in IV regression with DHS datasets. Table 6.0 column 1 reports our result. A year of schooling increases wealth by nearly 58 percent. This is more than three times the IV estimate reported using DHS dataset. In column 2, we incorporated three religion dummy variables to account for the potential effect that time invariant religious practices could have on wealth accumulation directly or indirectly through participation in the government funded UPE programme. Introducing these religion dummies increasing schooling coefficient slightly 59.16 percent and it is significant at 1 percent.

If UPE increased enrolment that has important consequences for variety of civic activities, including membership of political party in a society characterised by pervasive rent-seeking, observed wealth outcome might not because UPE beneficiaries performed better than related non-beneficiaries in the labour markets did. Rather, it could because UPE cohorts are using opportunities available to them in the political markets. While education leads to better labour market outcome because it makes people more productive, the experience in some developing countries shows public jobs are allocated to politically influential groups (Schultz, 2004).Reward system in essentially redistributive state is skewed in favour of the groups with political influence. In a recent study, Larreguy and Marshall (2013) finds that 1976 UPE has influenced a variety of political behaviours, including membership of political party. Therefore, it appears that the channel of causation is from UPE programme to increased schooling and then to political party membership, and to better wealth outcome. If this is the case in Nigeria, then our exclusion restriction condition is violated, and coefficients of schooling attainment questionable. To test whether this is responsible for observed outcome, dummy variable for political membership is included in our specification. As shown in column 3 Panel A, the coefficient of schooling attainment remains statistically significant at 1 percent.

However, omitted variables correlated with selection into schooling activities and with subsequent outcome variable could explain observed outcome. In the seminal study of Bowles and Gintis (1976), schooling is not only seen as contributing to labour market performance of new entrants by enhancing their cognitive capacity but by preparing people

for work place rules, fitting them into hierarchical structure of modern corporation and making them function properly in social settings. One other central proposition in their human capital theory argues that parents pass onto their children traits, which promote labour market success. This theory accounts for the significant great persistence in the intergenerational inequality found in a number of empirical studies between parents and children's adult economic status (see survey by Mulligan, 1997). Empirical studies surveyed in Mulligan (1997) report high estimates, of intergenerational coefficient, ranging from 0.34 to 0.68 for variables such as family income, wealth, income and earnings average.

Table 6 : IV REGRESSION RESULTS **ROBUSTNESS TO ALTERNATIVE SURVEY DATA**

Model	**1**	**2**	**3**	**4**	**5**	**6**
Panel A : Second Stage IV Regression (Dependent Variable: Log of Wealth)						
Schooling (years)	**0.5797*** (0.1052)**	**0.5916*** (0.1079)**	**0.5690*** (0.1038)**	**0.5681*** (0.1037)**	**0.5659*** (0.1030)**	**0.5658*** (0.1049)**
Religion Dummies	**No**	**Yes**	**Yes**	**Yes**	**Yes**	**Yes**
Political-Party Dummy	**No**	**No**	**Yes**	**Yes**	**Yes**	**Yes**
Parental Status	**No**	**No**	**No**	**Yes**	**Yes**	**Yes**
Koranic Education Dummy	**No**	**No**	**No**	**No**	**Yes**	**Yes**
Adult Education & OJT Dummies	**No**	**No**	**No**	**No**	**No**	**Yes**
Panel B: First Stage Regression (Dependent Variable: Schooling Attainment)						
UPE_E	**0.3079*** (0.0387)**	**0.3009*** (0.0383)**	**0.3160*** (0.0393)**	**0.3160*** (0.0393)**	**0.3171*** (0.0391)**	**0.3119 (0.0390)**
Observations	**139355**	**139355**	**131591**	**131591**	**131591**	**131094**
F-Statistics	**5222.51**	**4638.89**	**4118.41**	**3876.13**	**3867.62**	**3520.56**
Cragg-Donald Wald F statistic	**66.09**	**64.42**	**67.18**	**67.17**	**68.30**	**66.20**
Paap-Kleibergen F-Statistic	**63.30**	**61.85**	**64.53**	**64.52**	**65.76**	**63.90**

Baseline Variables in model 1 include Schooling Years, Age, Age-Squared, sector, gender, marital status dummy, infrastructure (railroad, distance to water source, dam dummy), ethnic capital, LGA or district FE and terrain ruggedness. Other association measures such as membership of religious, family, professional and other associations yield similar estimates as that reported in column 3. Robust standard errors are expressed in brackets. ***, **, and * indicate significance at the 1%, 5%, and 10% levels.

Unfortunately, the data on parental education and occupational status are essentially available for cohorts under the age of 20 years. Alternatively, the study estimate the percentage of 60 plus cohorts with post-primary education at the enumeration area level. This is what is used a

proxy for parental status. There are 10 enumeration areas (EA) in an LGA and 10 households in a given EA. This proxy is used based on the assumption that there is high degree of spatial segregation according to socio-economic status. Introducing this proxy barely any effect on the statistical significance of the coefficient of schooling attainment (column 4 Panel A).

Unfortunately, the data on parental education and occupational status are essentially available for cohorts under the age of 20 years. Alternatively, the study estimate the percentage of 60 plus cohorts with post-primary education at the enumeration area level. This is what is used a proxy for parental status. There are 10 enumeration areas (EA) in an LGA and 10 households in a given EA. This proxy is used based on the assumption that there is high degree of spatial segregation according to socio-economic status. Introducing this proxy barely any effect on the statistical significance of the coefficient of schooling attainment (column 4 Panel A).

The theoretical framework of Fryer (2007) shows that some ethnic groups tends to make certain kind of human capital investment which may facilitate or impede their success in the labour market. The theory posits that individual current human capital investment signals an intention to remain as part of her ethnic group or a desire to defect. When current investment is a litmus test of their loyalty to the group, the amount of schooling investment is dependent on whether the ethnic group in question considers schooling as desirable. Where it is considered undesirable, individuals are compelled to make investment in activities that are unrelated to labour market success. As at the time of independence, when schooling attainment in core north of Nigeria was extremely low, there were considerably large number of Koranic schools with equally significant enrolment rates. We will be under estimating the coefficients of schooling variable if koranic education reduces years of formal education and undermines labour market performance. Thus, we include a dummy variable for Koranic education in the specification reported in Panel B column 5. Coefficient of schooling attainment remain robust to the inclusion of this dummy variable.

Though coefficients of schooling attainment variable remain consistently significant at different specifications, there is a possibility that some other kind of education or skill acquisition may be driving outcome. For instance, adult education could be influencing wealth status as much as formal basic education acquired during childhood years. Rocha (2011) finds that adult education in Brazil increases individual wages by 21.25 percent but has negligible effect on individual employability[4].Okedara (1985) study of 3-year experimental literacy programme in Nigeria indicates a return to adult education of 10.5 percent.

It is important to note that considerable human capital investment takes place after formal schooling has ended (Heckman, Lochner, and Taber 1998). Much of this human capital acquisition is in the form of on-the-job training. While age variable captures part of the experience gained on the job, there is also the need to account for the potential impact of on-the-job training on wealth status. Dostie (2013) recent study shows firm-sponsored classroom training, enhances worker's productivity. Accounting for the potential impact of Koranic education and on-the-job training on wealth did not affect statistical significance of schooling coefficient (Column 5 Panel B).

[4]Schwerdt (2012) randomized field experiment reveals that adult education barely affects wages, employment and increased schooling in Switzerland.

While the literature is still controversial, school quality has been regarded as an important determinant of schooling attainment and labour market performance across developing and developed countries (Betts, 1995; Betts, 1996 and Grogger, 1996; Brunello and Checchi, 2005). If school quality changed after the introduction of the UPE programme, and the change is significantly related to current labour market performance, omitting school quality variable from the study's econometric model will violate IV exclusion restriction condition.

If we take into consideration the criticism that many of the school quality indicators used in a many studies are input-based, this study follows the approach suggested by Hanushek and co-researchers (Hanushek, 2003;Hanushek 2005; Hanushek and Kimko, 2000; Hanushek, and Woessmann, 2007;Hanushekand Woessmann, 2012). The approach involves using outcome-based performance rating in key areas such as reading, writing and quantitative skills as school quality indicators. Fortunately, the 2009 HNLSS data do have information on respondents' relative ability to read and write English language and perform simple computation without difficulty. This study used these as indicators of school quality[5]. If school quality is important and positively correlated with years of schooling and wealth, excluding school quality from our specification will understate the true impact of education on wealth status (Wooldridge, 2005).

As a first measure, the study examines the correlation between UPE exposure and ability to read, write and implement simple mathematical computation. We use the 2010 National Literacy Survey collected by National Bureau of Statistics. From the OLS and probit results presented in appendix 7, there is no statistically significant relationship between UPE indicator variable and ability to read, write and carry out simple computation. However, a positive and statistically significant relationship exist between UPE indicator and each measure of ability to read, write and do simple computation when HNLSS data are used (columns 1-3 Appendix 8). We use the 2008 National School Census Survey to estimate the percentage of schools established during the UPE program as at 1981 at the LGA level. High-intensity LGAs are those LGAs with values at and above mean and low-intensity LGAs are those with values below the mean. If UPE program caused a drop in school quality, schooling estimate from IV regression in low-intensity areas should be higher than similar estimate in high-intensity areas. However, results in columns 4 and 5 (Appendix 8) shows the wealth returns to schooling is 64.7 percent in high-intensity region and 51.2 percent in low-intensity area. Finally, we introduce ability to read dummy variable in column 6, dummies for reading and writing ability in column 7 and dummies for reading, writing and do simple computation in column 8 (appendix 8). Introducing these dummies dramatically increased the coefficients of schooling variable, making it less precise than before though still statistically significant at 1 percent.

6.0 Tackling Additional Instrument Validity Problems

In our reduced form specification, we find a strong positive relationship between the UPE instrument and wealth. The first-stage regressions in Tables 5 & 6 indicate positive and significant relationship between UPE instrument and endogenous schooling attainment variable. First and Second-stage IV regression estimates reported in Panels A and B Tables 5 & 6 buttress this fact. Our IV identification strategy is reliable to the extent that the schooling attainment is the only channel through UPE instrument affects welfare indicator. If this is true, then no positive relationship between false UPE instruments and wealth. For the same

[5] These are imprecise indicators of school quality and could as well as an indicator of natural ability.

reason, true UPE instrument should not have any positive and significant impact on wealth for countries that never implemented the UPE programme at the time Nigeria. If there is, then the validity of our true UPE instrument is in doubt. The luck of draw simply favoured our instrument as it is unlikely to be valid. It is possible that UPE instrument might simply be capturing trends in enrolment rates, without affecting schooling attainment (Oyelere, 2010). Bertrand et al (2004) ran placebo regressions with false instruments. They found significant impact of placebo interventions. Previously, Bound, Jaeger and Baker (1995) had shown that generating instruments that are random, even they are not relevant, could produce results similar to those reported in Angrist and Krueger (1991) that used season of birth and compulsory laws as instruments for schooling attainment.

To be sure our instruments were not spurious, we chose two sets of cohorts which were not exposed to 1976 UPE program and constructed two placebo UPE instruments for these cohorts. They include cohorts born before 1947 and those born within 1958-1964 who could not have benefited from regional UPE program. The first cohort UPE is negatively and significantly with wealth. The second has positive but statistically insignificant effect on wealth. We implemented a second set of placebo regressions. The variation in the implementation of UPE across Africa provides the appropriate platform to implement this test. Nigeria is probably one of few countries that introduced UPE programme in the mid-1970. We implemented placebo regressions for a select number of African countries with no history of UPE implementation in 1976. The countries include Senegal, Niger and Chad. For comparison, we run three OLS regressions for the pooled Nigeria 2008 and 2013 DHS data and separately for each year. DHS wealth index is our dependent variable (columns 3-5 appendix 9). In each specification, UPE indicator is positively and significantly correlated wealth index. In columns 6-8 (appendix 9), we report the results for three other African countries. There is positive though statistically insignificant relationship between the placebo UPE instrument and wealth index for Senegal, Niger and Chad. This suggests that our UPE instrument is probably exogenous.

Exposure to UPE should have positive and significant effect on schooling attainment. Since the implementation of the programme was at primary level of education, it should at least allow credit-constrained pupils to achieve basic education of six years. However, pupils from LGAs with high enrolment rates prior to the outset of the programme, who obviously comes from wealthier backgrounds, should achieve more years of post-primary education. According to Lang and Kropp (1986), those from richer families would want to distinguish themselves from credit-constrained pupils by having more than six years of education. Thus, there should be a positive and significant relationship between UPE indicator variable and dummies for the first six years of primary education. To test this insight, we develop six dummies for at least six years of primary, six dummies for at least years of secondary education and eleven dummies for at least 11 years of tertiary education. In appendix 10, the results of probit regression indicate a positive and significant between UPE indicator variable and the probability of completing at least six years of schooling. The contrasting results in appendix 11 is a negative and significant relationship between UPE indicator and probability of completing six years of secondary education for cohorts exposed to UPE program. Panels A and B of appendix 12 show insignificant relationship between UPE indicator and probability of ever completing at least the first 11 years of tertiary education. If the UPE programme were exogenous, it must affect the distribution of education in the population in a

specific way. If we must follow the prediction of standard human capital model, the UPE should affect only those below a required level of education (Lleras-Murray, 2002). Thus, the positive and significant relationship between UPE indicator variable and years of basic education is indicative of the fact the intervention might be exogenous.

Another important complication is that the state of the economy affects the demand and supply of education. The state can expand the supply of educational services during period of rapid economic growth and reduce supply when there is a downturn. For the same reason, demand for education by the households and individuals could be high during periods of rapid economic growth and low during downturns. Similarly, rapidly growingly economies will have more resources to finance education, particularly during boom periods. Thus, IV approach will produce biased results if UPE was introduced because the economy was growing rapidly and stopped because of a downturn.

Some have expressed the role oil wealth played in the initiation and spread of the UPE programme in Nigeria. The wealth from Nigeria's petroleum in the 1970s provided the country with considerable resources to carry out its UPE plan on a national scale (Bray, 1981). Rising life expectancy and drastic reduction in child mortality might also spur greater demand for education. Rational individuals, in the absence of constraints, might invest optimally in human capital when the reward for their investment will not be appropriated by the state. This occurs when the institutional environment will preclude political leaders from appropriating public resources for private use.

To provide additional evidence of instrument validity, we run a set of regressions that incorporate aggregate data into DHS data. We determined the year individuals in our sample reached the age of 6 years, the official school entry age as at 1976. Separately, we introduce annual real price of crude oil, under-5 mortality rate, life-expectancy, GDP growth rate, government as percentage of GDP, trade openness, exports as percentage of GDP and institutional quality in columns 2-10 (appendix 13). Column 1 reproduces IV regression reported in column 7 Table 5.0. IV coefficients remain significant at 1 percent, though they are less precisely estimated than before.

The macroeconomic and institutional environments at the time of labour market entry might also matter for observed wealth. Without information on the age of labour market entry for individuals, we assumed a labour market entry (LME) age of twenty years. We then introduced the same set of aggregate variables for the year individuals in our sample was 20 years. The reported results in appendix 14 shows that schooling estimates remain significant, though they are now less precisely estimated. We obtain similar results when LME is varied within 15-25 years. We also checked that entrance into the labour market before or after Structural Adjustment Programme (SAP) was responsible for observed outcome. SAP is a policy packaged introduced in 1986 by the Federal Government of Nigeria after the economic collapse of the 1980s. Assuming labour market entry age of between 15 and 25 years, we construct dummies, assigning 1 to those enjoined before 1986 when the market fairly favourable and zero to others when market situation had grown worse. As seen in columns 1-10 (appendix 15), schooling coefficients remain significant at 1 percent.

IV regressions often rely on the strong assumption that instruments are perfectly exogenous. However, this assumption is not likely to hold in reality. However, the question is what is the permissible limit of correlation between instrument and unobserved factors in the error term, which does not undermine confidence in the unbiasedness and consistency of the estimates of our key independent variable of interest? To determine the robustness of our IV estimates, we assume some correlation between our instruments and the error term so that we know the upper and lower limits of estimates.

To implement this strategy, we follow the suggestion of Conley, Hansen and Possi (2012) that allows the instrument to be incorporated into the second-stage regression of IV regression model. Their strategy accepts plausible rather perfect exogeneity, and implements econometric strategy under assumption of less than perfect correlation between instrument and the unobservables in the error term. Instrument of UPE exposure was incorporated into the second stage of the IV regression model, we can determine if the coefficient of schooling will include a value of zero for a specified confidence interval. We use the Union of Confidence Interval (UCI) and the much stricter Lower to Zero (LTZ) approach. In the UCI approach, coefficients of instrument in the reduced form regression are allowed to assume values of 0.01, 0.001 and 0.0001 at 95 percent confidence interval. As shown in appendix 16 (columns 1-3), our original IV estimate falls within the lower and upper limits of schooling variable reported. The LTZ approach produces similar results (column 4-6). Relaxing the assumption of perfect exclusion restrictions by allowing coefficients of instrument to be non-zero did not affect the estimate of schooling variable. Thus, IV results are still reliable. The mere fact that the coefficient obtained after accounting for imperfect orthogonality between UPE instrument and error term lies within acceptable intervals suggests that our instrument is plausibly exogenous.

Finally, we use one of the alternative measures of UPE instrument. The chosen UPE instrument is built on the assumption that school entry age is 5 years. This is allows us to implement formal test of over-identification test. This way, we can establish that our instrument is exogenous. This approach is useful because it is a direct test of exclusion restriction (Acemoglu, Johnson and Robinson, 2001). Column 7 reveals that schooling coefficient is comparable to estimate before now. This coefficient is also statistically significant at 1 percent. Result of overidentification test shows that our instruments are exogenous. Over-identification tests reinforce confidence in near exogeneity of chosen instruments. In the specifications reported in the last row of column 7, our results fail to reject the exclusion restriction condition by a wide margin. While this test is not definitive on the question of validity, it is at least assuring when taken alongside the results of other tests.

7.0 Comparison with Other Results.

Early studies used Net Present Value (NPV) approach in estimating benefits of private benefits of schooling. Psacharopoulos (1972) carried out a review of initial studies, indicating the benefits of students fall with rising level of education. In Nigeria, a serious attempt at deriving estimates of returns to education most likely started with Bowles (1965, 1967 and

1969) who restricted his interest to the Northern part of Nigeria[6]. Using the NPV approach, he asserts that the educational sector has a strong claim on economic resources and efficient allocation of resources within the educational system requires the expansion of primary education and a corresponding reduction in technical and secondary school education. In general, Bowles concluded that inadequate resources are devoted to education in Northern Nigeria. A similar estimation technique was used by Okedara (1985)[7], who studied the impact of three-year experimental adult literacy programme of the University of Ibadan on labour market performance in comparison with returns from formal schooling. The study reported 10.6 percent private returns to formal primary education.

Psacharoupoulos (1994)[8] reported OLS estimates of returns to primary, secondary and higher education of 23, 12.8 and 17.0 percent respectively based on a 1966 pre-tax survey data gathered from Western Nigeria. For Mid-Western Nigeria, Akangbou (1977) obtained OLS estimates of 13.4 percent for lower secondary school, 11.9 percent for vocational secondary, 11.2 percent for upper secondary school and 17.2 percent for university education. In a study by Fajana (1975), the vast inter-industry wage differential was attributed to economic and institutional factors. While high level of productivity in large-scale industrial outfit may partly account for the differentials, capital intensity of production and organized labour activities also significantly influence the wage differentials. Aromolaran (2004, 2006) who applied the OLS technique to the Mincer model obtained returns of between 2-4 percent for primary and secondary education and between 10-15 percent for tertiary education for between GHS covering 1997-1999. Okpako and Osakwe (1986) exploited the variation in the costs of schooling through the differences between mean expenditures, found the private internal rate of return to training as a nurse in Nigeria to be 146 percent.

In an exclusive study of manufacturing workers and firms in Nigeria, Soderbom and Teal (2002) report significantly high returns to schooling, experience and skill for this category of workers. The returns to schooling increases with rising years of schooling and with increasing size of firms. The second survey of the same sector produced similar results (Malik, Teal and Baptist, 2006). Though mainly interested in private returns to higher education, Okuwa (2004) also applied the OLS technique to the Mincer model and reported returns to extra year of schooling of 1.6% for secondary education, 12.7% for National Certificate of Education (NCE), 10.7% for polytechnics and 16.7% for university. Thus, contrary to the claims of Psacharoupoulos (1985, 1994 & 2004) for sub-Saharan Africa in general and Nigeria in particular, both studies showed that returns to education rise with the educational ladder. Note that both Aromolaran and Okuwa estimates could have under or over estimated returns to education because data constraint did not allow them to correct for self-selection bias. Amaghionyeodiwe and Osinubi (2007) report that schooling attainment and job experience are positively and significantly correlated with earnings. They report greater returns to schooling for those with higher education.

However, omitted variable bias due to unobserved ability remains controversial in estimating returns to education. Added to these are biases sue to endogeneity and measurement error. Because of the of endemic problems of schooling endogeneity and measurement error often associated with OLS econometric technique used in the studies reviewed by Psacharopoulos, more recent works are now being complemented by more modern and sophisticated econometric estimation techniques (Card, 1999). Now widely used is the Instrumental

[6] Bowles, Emeritus Professor of Economics at the University of Massachusetts, sent me a copy of the first article published from his 1965 Harvard PhD thesis in Economics and sent the title of the book which grew from his PhD work as well.
[7] This approach is flawed to the extent earnings reported as benefits may not be due to education.
[8] Reported in Psacharopoulos (1985) and then in Psacharopoulos and Patrinos (2004).

Variable (IV) approach.[9] The IV approach approximates randomized trial carried out in a natural experiment by natural scientists. Psacharopoulos and Patrinos (2004) acknowledge the increasing use of IV technique in the estimation of returns to education. In about half the number of cases, IV technique has not yielded estimates significantly higher than OLS estimates. In half the remaining studies, IV estimates have proved to be considerably higher than OLS estimates. This is probably because measurement error and omitted variables bias OLS estimates downward than endogeneity tend to raise these estimates.

A number of studies have used this IV technique in deriving the returns to education estimates, exploiting one natural experiments of nature[10] or change of government policy[11]. Card (1993) uses closeness of location of residence to college as an instrument for schooling. Butcher and Case (1994) argue that sibling sex composition is a good instrument for schooling because women without sisters have higher educational attainment than those with sisters. Harmon and Walker (1995) employ the quarter of birth and dummies connected to increases in compulsory schooling age. Angrist (1991), interested in measuring the long-term labour market consequences of military service, uses randomly assigned draft-lottery numbers as a suitable instrument. Angrist and Krueger (1991) adopt the season of birth as instrument because it is correlated with educational attainment due to age at the start of schooling and compulsory school attendance laws. The reasoning is that individuals born at the onset of one year begins school at an older age relative to those born later in the year. Thus, those born in the early part of the year have lesser school attainment than those born in the latter part of the year[12].

To correct for biases due to OLS technique, Oyelere (2010) used the IV technique employing similar datasets used by Aromolaran (2004, 2006) to derive estimates of between 3-3.6 percent for average year of education in Nigeria[13]. Though both Oyelere and Aromolaran report estimates that quantitatively similar, nevertheless Oyelere's upper limit of 3.6 percent against an upper limit of 15% in Aromolaran paper is particularly startling[14]. Oyelere's explanation for the low returns is that poor economic condition.

Though the use of IV technique represents an improvement over the use of OLS method. However, using wages as an outcome indicator in a developing country like Nigeria may have some complications. Therefore, the study used consumer durables as indicator of labour market performance, skipping the need to use income, which is often misreported and may be affected by short run fluctuations in economic activities. The few studies using the IV technique to evaluate UPE programme have not established a convincing case for instrument(s) exogeneity. The tendency is to assume perfect zero correlation between instrument and error term. Therefore, this study takes full advantage of the recent advancement in the studies of testing for IV exogeneity condition to establish the fact that UPE instruments are approximately exogenous. Thus, the returns to wealth reported for UPE beneficiaries in this study are higher than those reported in previous studies. The difference is not just because of differences in estimating technique and choice of data, but also because the returns reported here is exclusively for UPE beneficiaries. It is for this reason that we are

[9] To some extent, matching methods, control function methods, semi parametric estimations and non parametric estimation techniques are also being used.
[10] Examples include sibling-sex composition, proximity to college location, occurrence of twin births, quarter of birth etc.
[11] Examples include State laws, compulsory schooling laws, smoking, maximum class size rule, and discontinuities in financial aid formula among others.
[12] See Angrist and Krueger (2001:82) for a list of instruments used in empirical studies and detailed explanation of IV technique.
[13] She added more control variables than Aromolaran (2004) did. Control variables in her work included cohort, sex and location in addition to the fact that she used age and age squared in places of experience and experience squared to minimize measurement error and possible endogeneity of experience. An updated version of the same paper (Oyelere, 2009) reduced the estimate to 2.9%.
[14] Most of the result encountered usually report higher estimates for IV technique than for OLS method. See Girma and Kedir (2005) for another exception to the rule.

unable to compare OLS and IV estimates. To implement formal test of overidentification, we use one of the alternative instruments built on the assumption that the school entry age is five years.

8. Summary and Conclusion

In this study attempt to estimate the impact of schooling attainment on an important indicator of labour market performance: wealth. OLS and IV regressions produced economically and statistically significant estimates, with OLS estimate of about 18 percent and IV estimate of about 30 percent when pooled DHS is used and about 56 percent when HNLSS data are employed. We have no evidence that OLS estimates are an artefact of the way the dependent variable is constructed or influential observations are driving observed outcome. In our IV regression specification, econometric tests prove that instrument is strong. Indicative and formal tests of instrument validity such as addition of new relevant variables, falsification tests, plausibly exogenous test and over-identification test are proofs of instrument validity. A number of econometric strategies implemented indicate that influential observations and selective migration are not biasing our results.

While wealth as a proxy for individual wellbeing comes with its own limitations, a number of checks on this proxy justify its appropriateness in this context. One caveat to our results lies in our inability to instrument for explanatory variables such as migration, Koranic education, adult education and on-the-job training, which are obviously endogenous. According to Acemoglu and Angrist (2000), this could bias the estimates of our schooling attainment variable. However, we instrument for each of these endogenous variable one after the other.

Generally, the central contribution of this paper is to add to the ongoing effort at evaluating the 1976 UPE programme. Before now, rigorous econometric evaluation of the programme has not been implemented. The initial attempt at evaluating this programme effect on female schooling, child mortality and fertility has produced controversial results (Osili and Long, 2008: Osili, 2008: Palmer-Jones, 2008; Maret, 2012). Oyelere (2010) did not specifically evaluate the 1976 UPE programme, but used all UPE programmes implemented before and after political independence of Nigeria and 1976 UPE programme as instrument for schooling attainment. While the collective impact of the programme is substantial in terms of raising schooling attainment, it is small in terms of overall effects on individual income. From Oyelere study, we cannot determine the individual effects of all UPE programmes on schooling attainment and income. Thus, her study was in no way an evaluation of the UPE programme, but rather an attempt to use all UPE programmes to construct a valid instrument useful in resolving the identification problems associated with studies trying to derive causal estimates. It is at least fair to conclude that all things being equal, large-scale public sector investment in schooling is capable of increasing the wealth of Nigerian residents considerably.

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APPENDICES

Appendix 1: Unit Cost of Household Items in Naira

SN	ITEMS	COSTS (N)	SN	ITEMS	COSTS (N)
1	Bicycle	14588.24	**19**	Mattress	16759
2	CARS/VEHICLES	458272	**20**	Microwave	7750
3	Cassette recorder	3145.45	**21**	Mobile Phone	7023
4	Computer	57428.57	**22**	telephone	10000
5	DVD Player	5129.1	**23**	Motorbike	67159
6	Electric Clothes Dryer		**24**	Musical Instrument	5367
7	Fan	3858.1	**25**	Others (specify)	31220
8	Freezer	72071.43	**26**	Radio	3450
9	Fridge	53538.46	**27**	Satellite Dish	17325
10	Furniture (3/4 piece sofa set)	59179	**28**	Sewing machine	12170
11	Furniture (chairs)	16387	**29**	Stove (electric)	2500
12	Furniture (table)	6292	**30**	Stove (kerosene)	3861
13	Gas cooker	20000	**31**	Stove gas (table)	12500
14	Generator	126790	**32**	TV Set	16397
15	Hi-Fi (Sound System)	8500	**33**	Washing Machine	60000
16	Inverter	7089	**34**	Mattress	16759
17	Iron	8056	**35**	Microwave	7750
18	Mat	1488	**36**	Mobile Phone	7023

Data Source: National Bureau of Statistics, 2010.

Appendix 2: OLS Regressions

Variables/column	1	2	3	4	5	6
Data Sample	2006 GHS	2007 GHS	2008 GHS	2009 GHS	2008 DHS	2013 DHS
Schooling (years)	0.0019*** (0.0001)	0.0291*** (0.0032)	0.0417*** (0.0015)	0.1200 (0.0813)	0.2198*** (0.003)	0.1904*** (0.0023)
Age & Age-Squared	Yes	Yes	Yes	Yes	Yes	Yes
Sector Dummy	Yes	Yes	Yes	Yes	Yes	Yes
Gender Dummy	Yes	Yes	Yes	Yes	Yes	Yes
Household Size & Household Size Squared	No	No	No	No	Yes	Yes
Marital status dummy	Yes	Yes	Yes	Yes	Yes	Yes
Ethnic Capital	No	No	No	No	Yes	Yes
Time-Dummy	No	No	No	No	No	No
Observations	10924	14052	17124	3420	62195	72553
F-Statistics	79.19	35.61	304.37	1.70	2008.02	1913.18
R-Squared	0.0412	0.0146	0.0961	0.0012	0.1958	0.2000

Robust standard errors are expressed in brackets. ∗∗∗, ∗∗, and ∗ indicate significance at the 1%, 5%, and 10% levels.

Appendix 3: Controlling For More Variables (OLS Regressions)

Dependent Variable: Log of Wealth

Variables/column	1	2	3	4	5	6	7
Data Sample	2008 & 2013 DHS	2008 & 2013 DHS	2008 & 2013 DHS	2008 & 2013 DHS	2008 & 2013 DHS	2008 & 2013 DHS	2008 & 2013 DHS
Schooling (years)	0.2022*** (0.0018)	0.1942*** (0.0019)	0.2039*** (0.0018)	0.1933*** (0.0019)	0.1929*** (0.0019)	0.1919*** (0.0019)	0.1895*** (0.0019)
Other control variables	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Geopolitical Region Fixed Effects.	Yes	No	No	Yes	No	No	No
State Fixed Effects	No	Yes	No	Yes	No	No	No
LGA F.E	No	No	Yes	Yes	No	No	No
Latitude	No	No	No	No	Yes	No	No
Longitude	No	No	No	No	No	Yes	No
Altitude	No	No	No	No	No	Yes	Yes
Observations	134748	134748	134748	134748	134748	134748	134748
F-Statistics	3423.63	3435.73	3432.46	2921.95	2719.43	2588.59	2432.38
R-Squared	0.2093	0.2116	0.2105	0.2137	0.2138	0.2198	0.2210

Other control variables include age, age-squared, sector dummy, gender dummy, household size, household size squared, marital status dummy, ethnic capital and time dummy. To column 4 we add variables measuring geo-political region, state & LGA Fixed Effects. Robust standard errors are expressed in brackets. ∗∗∗, ∗∗, and ∗ indicate significance at the 1%, 5%, and 10% levels.

Appendix 4: More Robustness Tests (OLS Regressions)

Dependent Variable: Log of Wealth

Variables/column	1	2	3	4	5	6	7
Data Sample	Male Sample	Female Sample	Urban Sample	Rural Sample	Drop SW Observations	Drop Lagos Observations	Drop Abuja Observations
Schooling (years)	0.178*** (0.0025)	0.2087*** (0.00284)	0.1763*** (0.0023)	0.1972*** (0.0028)	0.1924*** (0.0021)	0.1922*** (0.0019)	0.1864*** (0.0019)
Other control variables	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	65616	69129	49109	85639	112343	129151	131486
F-Statistics	1221.21	1401.14	514.22	1107.32	1997.53	2271.85	2323.48
R-Squared	0.2257	0.2213	0.1935	0.1555	0.2096	0.2126	0.2163

Other control variables include age, age-squared, sector dummy, gender dummy, household size, household size squared, marital status dummy, ethnic capital and time dummy, fixed effects for geo-political region, state, LGA, LGA Latitude, LGA Longitude and LGA Ruggedness index. Dropping each of the other state one at time has not affected the statistical significance of the schooling coefficients. Similarly, implementing OLS regressions for each of the 36 states and Federal Capital Territory (F.C.T) produced coefficients of schooling variables that are statistically significant at 1 percent.
Robust standard errors are expressed in brackets. ***, **, and * indicate significance at the 1%, 5%, and 10% levels.

Appendix 5: Robustness to Alternative Measures of UPE Instrument Exposure Dummy & Instrument Strength Test

Variables/column	1	2	3	4	5	6	7	8
	Using Years of Exposure	Allowing School Entry Age To Be 5 Years	**Allowing School Entry To 7 Years**	**Overage And Underage (5 & 7 Years) As School Entry Age**	**Using All UPE Beneficiaries To Construct UPE Exposure Dummy**	**IV Maximum Likelihood Estimator To Check For Weak Instrument**	**UJIVE 1**	**UJIVE 2**
Panel A	**Dependent Variable: Log of Wealth**							
Schooling (years)	**0.3015*** (0.0633)**	**0.3268*** (0.0715)**	**0.2805*** (0.0632)**	**0.3030*** (0.0567)**	**0.3416*** (0.0491)**	**0.3022*** (0.0834)**	**0.3465*** (0.1104)**	**0.3453*** (0.1091)**
Other control variables	**Yes**	**Yes**	**Yes**	**Yes**	**Yes**	**Yes**	**Yes**	**Yes**
Panel B	**Dependent Variable: Schooling Attainment**							
UPE_E	**0.1445*** (0.0128)**	**0.3819*** (0.0370)**	**0.4438*** (0.0384)**	**0.4677*** (0.0362)**	**0.5294*** (0.0335)**	**0.3485*** (0.0394)**	**NA**	**NA**
Observations	**124656**	**124656**	**124656**	**124656**	**124656**	**124656**	**124656**	**124656**
F-Statistics	**10582.00**	**10580.86**	**10585.29**	**10598.80**	**10633.36**	**10568.85**	**2071.96**	**2073.63**
Cragg-Donald Wald F statistic	**141.54**	**115.22**	**145.49**	**180.93**	**270.71**	**NA**	**NA**	**NA**
Paap-Kleibergen F-Statistic	**126.45**	**106.80**	**133.35**	**166.57**	**249.20**	**NA**	**NA**	**NA**

Other control variables include age, age-squared, sector dummy, gender dummy, household size, household size squared, marital status dummy, ethnic capital and time dummy, fixed effects for geo-political region, state, LGA, LGA Latitude, LGA Longitude and LGA altitude. NA is not applicable. Robust standard errors are expressed in brackets. ***, **, and * indicate significance at the 1%, 5%, and 10% levels.

Appendix 6: IV Regression Results Robustness to Influential Observations & Selective Migration

Model	1	2	3	4	5	6	7	8
Survey	HNLSS	HNLSS	HNLSS	NDHS	NDHS	NDHS	HNLSS	HNLSS
Panel A: Dependent Variable: Log of Wealth	Drop FCT Obs.	Drop Lagos & FCT	Drop South West	Drop FCT Obs.	Drop Lagos & FCT	Drop South West	Selective Migration.	Non-Migrant Sample
Schooling (years)	0.1836*** (0.0590)	0.1851*** (0.0637)	0.1800** (0.0715)	0.3073*** (0.0859)	0.3064*** (0.0923)	0.2905*** (0.0958)	0.6070*** (0.1240)	0.5939*** (0.139)
Control Variables	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Migrant Dummy	No	No	No	No	No	No	Yes	No
Panel B: Dependent Variable: Schooling Years								
UPE_E	0.4860*** (0.0406)	0.4725*** (0.0419)	0.4495*** (0.0450)	0.3464*** (0.0397)	0.3374*** (0.0409)	0.3444*** (0.0436)	0.3290*** (0.0479)	0.3366*** (0.0542)
Observations	131355	125770	112237	121398	116464	105616	88845	68261
F-Statistics	11522.59	10395.19	8758.96	10270.03	9371.38	8226.12	2376.79	1778.61
Cragg-Donald Wald F statistic	152.91	136.562	108.894	82.61	74.24	68.45	49.00	40.33
Paap-Kleibergen F-Statistic	143.17	126.971	99.963	76.23	68.21	62.54	47.21	38.53

Baseline Variables in model 1 include Schooling Years, Age, Age-Squared, sector, gender, marital status dummy, infrastructure (railroad, distance to water source, dam dummy), ethnic capital, LGA or district FE and terrain ruggedness, three dummy variable for religion (other religion as base dummy), political party dummy, parental status, Koranic education dummy, adult education dummy, and on-the-job training dummy.

Robust standard errors are expressed in brackets. ∗∗∗, ∗∗, and ∗ indicate significance at the 1%, 5%, and 10% levels.

Appendix 7: Checking for School Quality & Ability-(2010 NATIONAL LITERACY SURVEY)

Column	**1**	**2**	**3**	**4**	**5**	**6**
Panel A: 2010 National Literacy Survey						
Regression Technique	**Ordered Probit**	**OLS**	**Ordered Probit**	**OLS**	**Ordered Probit**	**OLS**
Dependent Variables	**E4**	**E4**	**E5**	**E5**	E8B	E8B
UPE_E	**-0.00411 (0.0530)**	**0.0064 (0.0109)**	**0.0357 (0.0528)**	**0.0162 (0.0116)**	**0.0258 (0.0448)**	**-0.0027 (0.0110)**
Other Controls	**Yes**	**Yes**	**Yes**	**Yes**	**Yes**	**Yes**
Observations	**8980**	**8980**	**8975**	**8975**	**9405**	**9405**
Wald chi2(11)/F-Statistics	**981.48**	**103.95**	**1116.46**	**101.30**	**1746.50**	**28.36**
Pseudo R2/R-Squared	**0.1917**	**0.1484**	**0.1930**	**0.1543**	**0.0811**	**0.0425**

For Panel A other control variables include Highest Schooling attainment, Sector dummy, Sex dummy, Age, Age-Squared Marital Status, Christian dummy, Muslim dummy, Traditional religion dummy (Others are base dummy) (AGE RANGE 22-49 IN THE SAMPLE). Robust standard errors are expressed in brackets. ∗∗∗, ∗∗, and ∗ indicate significance at the 1%, 5%, and 10% levels.

E4 = ENGLISH LANG READING ABILITY (READ SMOOTHLY=3: READ WITH DIFFICULTY =2 CANNOT READ=1)
E5 = ENGLISH LANG WRITING ABILITY (WRITE SMOOTHLY=3: WRITE WITH DIFFICULTY =2 CANNOT WRITE=1)
E8B= COMPUTATION ABILITY (**CANT IDENTIFY FIGURE=1 : IDENTIFY FIGURE ONLY=2: DO COMPUTATION=3).**

Appendix 8: Checking for School Quality Using 2009 Harmonized National Living Standard Survey (HNLSS)

Model	1	2	3	4	5	6	7	8
Regression Technique	Probit	Probit	Probit	IV	IV	IV	IV	IV
Dependent Variables	Read	Write	Computation	Log of Wealth (High Intensity LGA sample)	Log of Wealth (Low Intensity LGA sample)	Log of Wealth	Log of Wealth	Log of Wealth
UPE_E	0.0528*** (0.0149)	0.0512*** (0.0145)	0.0530*** (0.0133)	-------	----------	--------	-------	----------
Schooling Years	NA	NA	NA	0.6467*** (0.1516)	0.5117** (0.2193)	1.3108*** (0.1917)	1.362*** (0.2035)	1.488*** (0.2363)
Read Dummy	No	No	No	No	No	Yes	Yes	Yes
Write Dummy	No	No	No	No	No	No	Yes	Yes
Computation Dummy	No	No	No	No	No	No	No	Yes
Other Controls	Yes	Yes	Yes	Yes	Yes	Yes		
Observations	153221	153221	153221	43066	45779	88845	88845	88845
Wald chi2(11)/F-Statistics	41585.07	49704.79	49053.34	448.10	903.85	9807.45	9800.84	9857.58
Pseudo R2/R-Squared	0.6309	0.6038	0.5341	-------	----------	--------	-------	----------
Cragg-Donald Wald F statistic				44.79	11.12	72.35	69.03	58.60
Kleibergen-Paap Wald rk F-Statistic				41.52	11.05	67.32	64.10	54.44

The control variables are schooling attainment, age, age-squared, gender dummy, sector dummy, marital status dummy, LGA fixed effects, LGA rail dummy, state dam dummy, ethnic capital, LGA terrain ruggedness, Christian dummy, Muslim dummy, traditional religion dummy, koranic education dummy, adult education dummy and on-the-job training dummy. Robust standard errors are expressed in brackets. ***, **, and * indicate significance at the 1%, 5%, and 10% levels.

Read = English Lang Reading Ability (can read smoothly=1: otherwise zero)
Write = English Lang Writing Ability (can write smoothly=1: otherwise zero)
Computation= Computation Ability (**can do computation=1, otherwise zero)**

Appendix 9: Falsification Tests Results (2008 & 2013 Demographic & Health Surveys)

Model	1	2	3	4	5	6	7	8
Survey	(COHORTS BORN ON OR BEFORE 1947)	(USING COHORTS 1958-1964)	NIGERIA NDHS	2008 NDHS	2013 NDHS	2005 & 2011 Senegal DHS	2006 & 2012 Niger DHS	2004 CHAD DHS
Dependent Variables	Log of Wealth	Log of Wealth	Wealth Index	Wealth Index	Wealth Index	Wealth Index	Log of Wealth	Log of Wealth
UPE_F	-1.5280*** (0.0380)	0.0167 (0.0295)	----------	-------	----------	--------	-------	----------
UPE_E	----------	----------	4567.4*** (709.9)	7004.1*** (1010.4)	1965.9** (1004.6)	768.98 (1491.7)	828.13 (1189.9)	394.4 (2203.7)
Other Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	136533	136533	140135	64955	75180	30681	40394	29132
F-Statistics	3354.45	3138.35	10812.75	5719.97	6917.66	3363.26	7512.24	4866.33
R-Squared	0.1973	0.1829	0.3507	0.3457	0.3557	0.4341	0.5273	0.4551

Regressions reported in columns 1-2 contain the complete of covariates employed in the regression in column 7 Table 2.0. The control variables used in regressions shown in column 3-8 include schooling attainment, age, age-squared, gender dummy, sector dummy, marital status dummy. Robust standard errors are expressed in brackets. ∗∗∗, ∗∗, and ∗ indicate significance at the 1%, 5%, and 10% levels.

Appendix 10: Probit Regression Results (2008 & 2013 Demographic & Health Survey)

Model	**1**	**2**	**3**	**4**	**5**	**6**
Dependent Variable	Pry 1	Pry 2	Pry 3	Pry 4	Pry 5	Pry 6
UPE_E	**0.1671*** (0.0126)**	0.1677*** (0.0126)	**0.1705*** (0.0126)**	**0.1686*** (0.0125)**	**0.1702*** (0.0124)**	**0.1506*** (0.0123)**
Other Controls	**Yes**	**Yes**	**Yes**	**Yes**	**Yes**	**Yes**
Observations	**139182**	**139182**	**139182**	**139182**	**139182**	**139182**
Wald chi2(10)	**32286.76**	**32297.50**	**32329.08**	**31969.62**	**31772.39**	**31452.46**
Pseudo R2	**0.2762**	**0.2770**	**0.2775**	**0.2724**	**0.2693**	**0.2638**

The dependent variables (Pry 1-6) are dummy variables expressed as at least 1, 2, 3, 4, 5 and 6 years of primary schooling. For columns 1 and 2, control variables include gender dummy, sector dummy, marital status dummy, ethnic capital, time dummy time to reach water source in minutes, electricity available dummy, LGA fixed effects and LGA altitude. Robust standard errors are expressed in brackets. ***, **, and * indicate significance at the 1%, 5%, and 10% levels.

Appendix 10: Probit Regression Results (2008 & 2013 Demographic & Health Survey)

Model	**1**	**2**	**3**	**4**	**5**	**6**
Dependent Variable	Pry 1	Pry 2	Pry 3	Pry 4	Pry 5	Pry 6
UPE_E	**0.1671*** (0.0126)**	0.1677*** (0.0126)	**0.1705*** (0.0126)**	**0.1686*** (0.0125)**	**0.1702*** (0.0124)**	**0.1506*** (0.0123)**
Other Controls	**Yes**	**Yes**	**Yes**	**Yes**	**Yes**	**Yes**
Observations	**139182**	**139182**	**139182**	**139182**	**139182**	**139182**
Wald chi2(10)	**32286.76**	**32297.50**	**32329.08**	**31969.62**	**31772.39**	**31452.46**
Pseudo R2	**0.2762**	**0.2770**	**0.2775**	**0.2724**	**0.2693**	**0.2638**

The dependent variables (Pry 1-6) are dummy variables expressed as at least 1, 2, 3, 4, 5 and 6 years of primary schooling. For columns 1 and 2, control variables include gender dummy, sector dummy, marital status dummy, ethnic capital, time dummy time to reach water source in minutes, electricity available dummy, LGA fixed effects and LGA altitude. Robust standard errors are expressed in brackets. ***, **, and * indicate significance at the 1%, 5%, and 10% levels.

Appendix 11: Probit Regression Results (2008 & 2013 Demographic & Health Survey)

Model	**1**	**2**	**3**	**4**	**5**	**6**
Dependent Variable	**Sec 1**	**Sec 2**	**Sec 3**	**Sec 4**	**Sec 5**	**Sec 6**
UPE_E	**-0.0687*** (0.01197)**	**-0.0658*** (0.0120)**	**-0.0539*** (0.0121)**	**-0.0548*** (0.0122)**	**-0.0528*** (0.0122)**	**-0.0333*** (0.0142)**
Other Controls	**Yes**	**Yes**	**Yes**	**Yes**	**Yes**	**Yes**
Observations	**139182**	**139182**	**139182**	**139182**	**139182**	**139182**
Wald chi2(10)	**28735.40**	**28280.16**	**27126.87**	**26619.69**	**25547.67**	**10334.59**
R-Squared	**0.2335**	**0.2271**	**0.2144**	**0.2089**	**0.1984**	**0.0986**

The dependent variables (Sec 1-6) are dummy variables expressed as at least 1, 2, 3, 4, 5 and 6 years of secondary schooling. Control variables include gender dummy, sector dummy, marital status dummy, ethnic capital, time dummy time to reach nearest water source in minutes, electricity available dummy, LGA fixed effects and LGA altitude. Robust standard errors are expressed in brackets. ∗∗∗, ∗∗, and ∗ indicate significance at the 1%, 5%, and 10% levels.

Appendix 12: Probit Regression Results (2008 & 2013 Demographic & Health Survey)

Panel A						
Model	**1**	**2**	**3**	**4**	**5**	**6**
Dependent Variable	**Ter1**	**Ter 2**	**Ter 3**	**Ter 4**	**Ter 5**	**Ter 6**
UPE_E	**-0.0311** (0.0143)**	**-0.0464*** (0.0151)**	**-0.0619*** (0.0169)**	**-0.0142 (0.0244)**	**0.0075 (0.0277)**	**0.0230 (0.0311)**
Other Controls	**Yes**	**Yes**	**Yes**	**Yes**	**Yes**	**Yes**
Observations	**139182**	**139182**	**139182**	**139182**	**139182**	**139182**
Wald chi2(10)	**9222.29**	**6243.25**	**4484.07**	**1117.45**	**1055.06**	
R-Squared	**0.0920**	**0.0748**	**0.0760**	**0.0373**	**0.0432**	
Panel B						
Dependent Variable	**Ter7**	**Ter 8**	**Ter 9**	**Ter 10**	**Ter 11**	
UPE_E	**0.0348 (0.0326)**	**0.0421 (0.0332)**	**0.0398 (0.0333)**	**0.0394 (0.0333)**	**0.0392 (0.0333)**	
Other Controls	**Yes**	**Yes**	**Yes**	**Yes**	**Yes**	
Observations	**139182**	**139182**	**139182**	**139182**	**139182**	
Wald chi2(10)	**1239.26**	**1270.19**	**1274.70**	**1274.44**	**1273.03**	
R-Squared	**0.0641**	**0.0671**	**0.0678**	**0.0678**	**0.0677**	

The dependent variables (Ter 1-11) are dummy variables expressed as at least 1, 2, 3, 4, 5, 6, 7, 8, 9, 10 and 11 years of tertiary schooling. Control variables include gender dummy, sector dummy, marital status dummy, ethnic capital, time dummy time to reach water source in minutes, electricity available dummy, LGA fixed effects and LGA altitude.
Robust standard errors are expressed in brackets. ∗∗∗, ∗∗, and ∗ indicate significance at the 1%, 5%, and 10% levels.

Appendix 13: IV Regressions Results Controlling For Macroeconomic Conditions When Individuals Was Age 6 (2008 & 2013 Nigerian Demographic & Health Survey Data)

Panel A: Second Stage Results (Dependent Variable: Log of Wealth)										
Model	**1**	**2**	**3**	**4**	**5**	**6**	**7**	**8**	**9**	**10**
Schooling (years)	**0.3022*** (0.0834)**	**0.3593*** (0.1324)**	**0.4229*** (0.1427)**	**0.3851*** (0.1464)**	**0.4422*** (0.1402)**	**0.5681*** (0.1974)**	**0.3177*** (0.1252)**	**0.4212*** (0.1334)**	**0.4083*** (01458)**	**0.4157*** (0.1436)**
Other Controls	**Yes**	**Yes**	**Yes**	**Yes**	**Yes**	**Yes**	**Yes**	**Yes**	**Yes**	**Yes**
Macro-variable added (annual)	**None**	**Crude Oil Price**	**Under 5 Morality Rate**	**Life Expectancy**	**Annual GDP Growth Rate**	**Govt Expd As % of GDP**	**Trade Openness**	**Inflation**	**Exports As % of GDP**	**Institution**
Panel B: First Stage Results (Dependent Variable: Schooling Attainment (Measured in Years)										
UPE_E	**0.3485*** (0.0394)**	**0.2172*** (0.0386)**	**0.2029*** (0.0381)**	**0.1957*** (0.0381)**	**0.2138*** (0.0391)**	**0.1735*** (0.0419)**	**0.2461*** (0.0427)**	**0.2174*** (0.0383)**	**0.2404*** (0.0467)**	**0.2011*** (0.0381)**
Observations	**124656**	**109258**	105993	105993	105993	105993	105993	105993	105993	105993
F-Statistics	**11997.52**	**8529.94**	**7926.00**	**7925.27**	**7924.21**	**7924.15**	**7923.38**	**7922.23**	**7924.42**	**7929.87**
Cragg-Donald Wald F statistic	**84.585**	**34.07**	**30.11**	27.20	**31.96**	**18.34**	**35.71**	**38.03**	**28.40**	**29.56**
Paap-Kleibergen F-Statistic	**78.132**	**31.94**	**28.34**	25.83	**29.97**	**17.14**	**33.26**	**36.27**	**26.48**	**27.84**

Other control variables include age, age-squared, sector dummy, gender dummy, marital status dummy, ethnic capital, time dummy, infrastructure capital (electricity availability dummy and time to get to water source), LGA fixed effects, geopolitical and state fixed effects, and geographic variables (latitude, longitude and altitude). Robust standard errors are expressed in brackets. ***, **, and * indicate significance at the 1%, 5%, and 10% levels.

Appendix 14: IV Regressions Results Controlling For Macroeconomic Conditions When Individuals Was Age 20 (From Pooled 2008 & 2013 Nigerian Demographic & Health Survey Data)

Panel A: Second Stage Results (Dependent Variable: Log of Wealth)										
Model	**1**	**2**	**3**	**4**	**5**	**6**	**7**	**8**	**9**	**10**
Schooling (years)	**0.3022*** (0.0834)**	**0.3488*** (0.0953)**	**0.4233*** (0.1158)**	**0.3805*** (0.1112)**	**0.5415*** (0.1309)**	**0.3804*** (0.1175)**	**0.3836*** (0.1318)**	**0.4235*** (0.1420)**	**0.3898*** (0.1167)**	**0.5687*** (0.1414)**
Other Controls	**Yes**	**Yes**	**Yes**	**Yes**	**Yes**	**Yes**	**Yes**	**Yes**	**Yes**	**Yes**
Macro-variable added (annual)	**None**	**Crude Oil Price**	**Under 5 Morality Rate**	**Life Expectancy**	**Annual GDP Growth Rate**	**Govt Expd As % of GDP**	**Trade Openness**	**Inflation**	**Exports As % of GDP**	**Institution**
Panel B: First Stage Results (Dependent Variable: Schooling Attainment (Measured in Years)										
UPE_E	**0.3485*** (0.0394)**	**0.2861*** (0.0357)**	**0.2466*** (0.0369)**	**0.2504*** (0.0363)**	**0.2486*** (0.0383)**	**0.2423*** (0.0367)**	**0.2161*** (0.0371)**	**0.2097*** (0.0385)**	**0.2414*** (0.0368)**	**0.2359*** (0.0383)**
Observations	**124656**	117828	117828	117828	117828	117828	117828	117828	117828	117828
F-Statistics	**11997.52**	**9418.54**	**9173.19**	**9173.66**	**9169.00**	**9172.31**	**9183.02**	**9168.80**	**9172.67**	**9171.15**
Cragg-Donald Wald F statistic	**84.585**	**69.25**	**47.65**	**51.26**	**44.54**	**47.10**	**36.84**	**32.29**	**46.39**	**131.429**
Paap-Kleibergen F-Statistic	**78.132**	**64.24**	**44.56**	**47.55**	**42.12**	**43.35**	**34.00**	**29.64**	**42.96**	**126.125**

Other control variables include age, age-squared, sector dummy, gender dummy, marital status dummy, ethnic capital, time dummy, infrastructure capital (electricity availability dummy and time to get to water source), LGA fixed effects, geopolitical and state fixed effects, and geographic variables (latitude, longitude and altitude). Robust standard errors are expressed in brackets. ***, **, and * indicate significance at the 1%, 5%, and 10% levels.

Appendix 15: Testing the Impact of Labour Market Entry (LME) Before & After 1986 SAP Programme Assuming Different Ages of LME (From Pooled 2008 & 2013 Nigerian Demographic & Health Survey Data)

Panel A: Second Stage Results (Dependent Variable: Log of Wealth)											
Model	**1**	**2**	**3**	**4**	**5**	**6**	**7**	**8**	**9**	**10**	**11**
Schooling (years)	**0.2483*** (0.0522)**	**0.2871*** (0.0548)**	**0.4025*** (0.0780)**	**0.5094*** (0.1307)**	**0.4146*** (0.1102)**	**0.4194*** (0.1517)**	**0.4849** (0.2232)**	**0.3937 (0.2524)**	**0.4930 (0.3400)**	**0.2197 (0.1509)**	**0.1836 (0.1573)**
Other Controls	**Yes**	**Yes**	**Yes**	**Yes**	**Yes**	**Yes**	**Yes**	**Yes**	**Yes**	**Yes**	**Yes**
LME (SAP)	**15**	**16**	**17**	**18**	**19**	**20**	**21**	**22**	**23**	**24**	**25**
Panel B: First Stage Results (Dependent Variable: Schooling Attainment (Measured in Years)											
UPE_E	**0.5571*** (0.0416)**	**0.5827*** (0.0449)**	**0.4990*** (0.0519)**	**0.3052*** (0.0508)**	**0.3155*** (0.0467)**	**0.2258*** (0.0461)**	**0.1557*** (0.0452)**	**0.1285*** (0.0444)**	**0.0994** (0.0438)**	**0.1958*** (0.0427)**	**0.1855*** (0.0425)**
Observations	**134613**	134613	134613	134613	134613	134613	134613	134613	134613	134613	134613
F-Statistics	**11098.10**	**11093.67**	**11087.99**	**11090.51**	**11099.47**	**11121.01**	**11158.81**	**11194.89**	**11231.62**	**11234.45**	**11268.96**
Cragg-Donald Wald F statistic	**196.77**	**178.36**	**95.57**	**37.83**	**48.532**	**25.65**	**12.72**	**9.03**	**5.56**	**22.69**	**20.69**
Paap-Kleibergen F-Statistic	**182.60**	**168.62**	**92.47**	**36.10**	**45.708**	**24.03**	**11.85**	**8.38**	**5.14**	**21.00**	**19.11**

LME is labour market entry age. Indicator variables are developed from SAP as age at 1986 minus assumed LME age. Positive values are assigned 1, otherwise zero. Other control variables include age, age-squared, sector dummy, gender dummy, marital status dummy, ethnic capital, time dummy, infrastructure capital (electricity availability dummy and time to get to water source), LGA fixed effects, geopolitical and state fixed effects, and geographic variables (latitude, longitude and altitude). Robust standard errors are expressed in brackets. ***, **, and * indicate significance at the 1%, 5%, and 10% levels.

Appendix 16: Robustness to Imperfect Instrument Tests (From Pooled 2008 & 2013 Nigerian Demographic & Health Survey Data)

Panel A: Second Stage Results (Dependent Variable: Log of Wealth)								
Model	**1**	**2**	**3**	**4**	**5**	**6**	**7**	**8**
Method	**IV**	**UCI (0--0.01)**	**UCI (0--0.001)**	**UCI (0--0.0001)**	**LTZ (U=0.1 & sd=0.05)**	**LTZ(u=0.01 & sd=0.005)**	**LTZ(u=0.001 & sd=0.0005)**	**IV**
Schooling (years)	**0.3022*** (0.0834)**	**NA**	**NA**	**NA**	**0.1565 (0.1679)**	**0.2876*** (0.0846)**	**0.3007*** (0.0834)**	**0.3320*** (0.0707)**
Schooling (years) Lower Bound	**NA**	**-0.1457**	**0.1114**	**0.1360**	**-0.1726**	**0.1217**	**0.1373**	**NA**
Schooling (years) Upper Bound	**NA**	**0.4656**	**0. 4656**	**0.4656**	**0.4855**	**0.4535**	**0.4642**	**NA**
Other Controls	**Yes**	**Yes**	**Yes**	**Yes**	**Yes**	**Yes**	**Yes**	**Yes**
Panel B: First Stage Results (Dependent Variable: Schooling Attainment (Measured in Years)								
UPE_E	**0.3485*** (0.039)**	**NA**	**NA**	**NA**	**NA**	**NA**	**NA**	**0.1000 (0.0904)**
UPE_E5	**NA**	**NA**	**NA**	**NA**	**NA**	**NA**	**NA**	**0.4667*** (0.0847)**
Observations	**124656**	**124656**	**124656**	**124656**	**124656**	**124656**	**124656**	**124656**
F-Statistics	**10568.85**	**NA**	**NA**	**NA**	**NA**	**NA**	**NA**	**9959.15**
Cragg-Donald Statistic	**84.58**	**NA**	**NA**	**NA**	**NA**	**NA**	**NA**	**58.26**
Paap-Kleibergen F-Statistic	**78.13**	**NA**	**NA**	**NA**	**NA**	**NA**	**NA**	**54.13**
OVERID	**NA**	**NA**	**NA**	**NA**	**NA**	**NA**	**NA**	**0.4572**

NA is not applicable. UCI is Union of Confidence Interval approach & LTZ is Local to Zero approach. In the UCI method the assumption of perfect exogeneity between instrument and error is replaced with near perfect exogeneity with assumed correlation values of 0.01, 0.001 and 0.0001 for columns 2-4. U is mean and s.d is standard deviation. In the LTZ approach, we specify the entire distribution (in terms of mean and standard deviation) which the coefficient of the plausibly exogenous variable can take as (u=0.1 & s.d=0.05), (u=0.01 & s.d=0.005) and (u=0.001 & s.d =0.0005) in columns 5-7. Other control variables include age, age-squared, sector dummy, gender dummy, marital status dummy, ethnic capital, time dummy, infrastructure capital (electricity availability dummy and time to get to water source), LGA fixed effects, geopolitical and state fixed effects, and geographic variables (latitude, longitude and altitude). Robust standard errors are expressed in brackets. ***, **, and * indicate significance at the 1%, 5%, and 10% levels.